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THE ORIGIN AND ECONOMY OF ENERGY IN THE UNIVERSE

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"And, behold, the bush burned with fire, and the bush was not consumed."—Exodus, Ch. III, v. II.

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By Dr. ISRAEL KAUFMAN

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The Chief Press, New York.

PREFACE.

Seventeen hundred and fifty-eight was a year of eager expectation and watchfulness for astronomers in particular and for mathematicians and scientists in general, all the world over. Halley's comet, the return of which for that year (1758) was for the first time, in the history of astronomy, predicted by Halley himself, many years before, was impatiently expected. Great results were at stake. Not only the curious demonstration of the great, almost prophetic, power of the human mind, but a greater result than this: no less than a positive verification and proof of the great theory of the great Newton—the theory of Universal Gravitation, which at that time had some opponents.

No wonder, then, that, in the language of Sir John Hershel, "all the astronomers furbished up their telescopes, and all the mathematicians set to work to calculate," as the time of the predicted return drew near.

But although "all the astronomers of Europe were looking out for it, eager to seize it on its first coming within the range of human vision"—still—"they were all disappointed of their prize. It was carried off by a Saxon farmer of the name of Palitsch, an astronomer of Nature's own creating, who was always watching the heavens—without telescopes, without knowledge—simply from the profound interest their aspect inspired him with. He, it was, who first caught sight of it on the 13th December, 1758." (See *Familiar Lectures on Scientific Subjects,* by Sir John Hershel. *Comets.*)

This incident with farmer Palitzch ought to serve us as a good and instructive lesson, that even men "without telescopes, without knowledge," should, in the interest of truth, science, and humanity, be en-

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titled to a fair hearing of what they persistently claim.

The writer of the following pages humbly places himself within the last category of men. He, with humility and almost with reluctance, claims to have discovered a great central truth of Nature—of cosmical Nature—of world-wide importance; a truth, which for long, long centuries lay hidden from the mental gaze of mankind, deep within the unfathomable treasury of Nature. The lack of knowledge of this truth, so the author claims, necessarily led the minds of men to great misconceptions with reference to Nature and her workings. It, the lack of knowledge of this truth, rendered the universe, in our view, temporary, inconstant, imperfect, and far from uniform. More, her grandest operations and phenomena (in a cosmical sense) became, in the absence of this truth, causeless and inexplicable—great mysteries of Nature, to solve which all kinds of conceivable and inconceivable hypotheses were, from time to time, advanced by various great men—only to be supplanted by other similar hypotheses later on. Witness:—

1. The mystery of the strange phenomena displayed by comets.

2. The great mystery of the sun: its heat; solar spots; their periodicity; equatorial acceleration, and many others.

3. The great mystery of the phenomena displayed by the stars: their binary nature; the meaning and function of dark and invisible stars; the variability of stars; their periodicity, etc.

4. The mystery of nebulæ: their meaning; source of energy radiated; their motion, etc.

5. Some mysterious phenomena connected with the earth's plutonic energy: cause of eruptions; time and situation, etc.

6. Some phenomena connected with planets and the moon.

7. Inexplicable phenomena connected with meteors and shooting stars.

All these mysteries, and many more, are, so the author claims, rendered clear and transparent by *that single* truth or law of Nature. More, that law given, and all these mysterious phenomena become a necessity; and on the sertngth of *it* the existence of such phenomena might and must be inferred, even prior to a knowledge of their actual existence. As a matter of fact, the existence of what is now called "spectroscopic binaries" was inferred by the author as a necessary corollary to *that* law before he ever learned that such have actually been discovered by Vogel, Campbell, and others. Similarly, the greater activity of the earth's plutonic energy during the winter months has also been inferred by the author on the strength of that law, before he ever knew that such is actually the case.

In a word, that law given, and the universe becomes spotless, perfect, permanent, constant, and uniform throughout—and free from all contradiction and mystery.

But a great difficulty had to be encountered. *That* law absolutely demanded a remodelling, a restatement, so to speak, of some of the most fundamental principles of physical science—the grandest and most sublime generalizations and laws of force and energy, motion and inertia; among which the grand law of *conservation of energy* stands out most prominently. I said 'a remodelling' advisedly—not a complete denial of any of them, but a remodelling, a restatement of them in strict and perfect accordance with the facts of Nature, upon which these grand generalizations rest. A restatement which shall contain *no more* and *no less* than the facts, upon which it rests, justify; which shall tell us *no more* and *no less* than those facts themselves declare. In fact, the newly discovered law, as claimed by the humble writer of this, was suggested to him, in the first instance, not by an examination of mysterious or now-mysterious cosmical phenomena, to which this law supplied a missing explanation; but by a thorough examination and analysis of those very firmly established fundamental principles and laws of force and energy, motion and inertia, as understood at present. These very principles appeared to him to be somewhat hazy and ill defined; and a thorough critical examination and re-examination of them seemed to reveal the most startling and almost incredible fact that they, those very principles, contained within themselves a most insurmountable contradiction.

By "a great difficulty had to be encountered" is not meant, however, the difficulty of execution—of effecting that remodelling or restatement. This, though in itself a great difficult task, indeed, did not present itself so insurmountable as a certain difficulty of quite another sort: namely, that of overcoming a certain intense, though undefinable, feeling which took full possession of the heart whenever the mind reverted to the subject.

For, for a man "without telescopes, without knowledge" unceremoniously to enter the great, holy temple of science with such intention—intention of disturbing the great corner stones upon which it is built—stones that have been laid by the greatest men in the history of science: Newton, Galileo, Huyghens, Leibnitz, Des Cartes, and others—seems not only to be presumptuous in the extreme, but something akin to profanity and sacrilege. Hence that undefinable feeling and extreme difficulty of overcoming it.

But, as re-examination after re-examination for many times, and in a most thorough manner, of the premises involved, convinced the writer more and more of the positive truth and necessity of the inferable law; and, moreover, as on applying this very law first to the solar system and then to the entire universe and its physical phenomena, he was delightfully struck to find that it at once sheds a dazzling flood of brilliant light on them all, illuminating the entire cosmos even to its darkest corners and recesses, and that all mystery, contradiction, and confusion are banished by it from the clear face of pure and spotless Nature—he felt it his moral duty, in the interest of truth, science, and humanity, to lay aside all personal feelings, motives, and all other considerations, and to hesitate no longer in undertaking this very serious and difficult task, happen what may. The result is, the present volume, with its merits and demerits, just as it is.

I have written all this for two reasons: first, to obtain a fair and unprejudiced hearing, and, secondly, to defend myself against the charge of undervaluing authority, or treating lightly of established principles and the opinions of those to whom or to whose memory mankind, undoubtedly, ought to, and does look with great reverence and with a sense of the deepest gratitude.

Should the language employed in the course of any argument, in any part of this work, appear to the kind and indulgent reader to justify such implication, he is here most earnestly asked not to attribute this to levity and want of respect for authority on the part of the writer, but to the heat of the argument, or to the nature of the subject—for such is the bare truth.

BROOKLYN, N. Y. MAY 10, 1904.

The Thesis of this Volume is stated on p. 2

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PART I.

ARGUMENT.

BOOK I.

FORCE
AND
ENERGY.

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BOOK I. FORCE AND ENERGY.

SECTION I.—FORCE.

1. The absolute nature of force, like the absolute nature of matter, being unknowable, no essential definition of the term force is possible. And the question "what is force," in its essence, must forever remain unanswered.

Herbert Spencer, in his first principles, derives the ideas of space, time, motion, and matter from the idea of force; and the idea of force, itself, he, rightly, calls the ultimate of all ultimates. That is to say, we can define matter, time, space, and motion in terms of *force;* but force itself, being ultimate and non-derivative, admits of no definition in other terms but its own. Consequently, even admitting with Herbert Spencer, that in consciousness we have some indefinite notion of the essential nature of force, still, a definition of it is impossible on account of lack of terms in which to define it.

2. Though, as it was just said, we know nothing or next to nothing of the *essence* of force, we, nevertheless, are perfectly positive of its existence; that is to say, we are perfectly convinced that there exists an objective entity corresponding to our subjective idea of force.

And of the existence of force we know, certainly not through its essence, but through its effects on our consciousness. Now, if we examine the nature of the effects of force on our consciousness, we find one attribute common to them all, and that is *change.*

3. From what has been said, it is evident that force can be defined only by the sign of its existence:

that is, by its effects, which are generally of the nature of change. The following definition of force, then, seems to be the only proper one possible:—Force is that existing *something* which produces or tends to produce various changes in things objective, and through them in the subjective states of our consciousness.

4. But since "change" in the physical world is inconceivable without "motion," molar or molecular, the two terms are physically interchangeable; and in the above definition we may substitute "motion" in place of "change." It will then read, abbreviated, as follows:—force is that which produces or tends to produce motion. This definition includes change of velocity and change of direction, as indicative of force; for, is not change of velocity and change of direction the result of the production of new motion?

5. The above definition ought to, and does, embrace all kinds of physical forces:—attractive, repulsive, chemical, magnetic, electric, etc. All of these are termed forces, because they produce or tend to produce motion, as everyone may convince himself by analyzing each of the above ennumerated forces.

There is one class of forces, however, that do not produce nor tend to produce motion. This class consists of resisting forces which not only do not produce motion but even hinder it, such as inertia and cohesion. These are, properly speaking, *forces,* because they produce "change." But a great distinction is to be drawn between these passive or static forces and the active or dynamic forces, as will be more fully explained further.

6. The effects of forces must be conceived as forces themselves. For, by the above definition, the

effects of forces are, necessarily, motion; and since these motions are capable of being communicated to other bodies, consequently, they must be looked upon as forces.

7. Physicists classify forces in various ways, of one of which we will take notice here.—

According to their mode of action, with respect to time, forces are generally spoken of as (1) Impulsive and (2) Continuous.

The propulsion of a ball from a gun-barrel by the explosion of gun powder, is an example of the first; gravitation, of the second.

Now, we wish to remark, with respect to the so-called continuous forces, that their mode of action is in strict reality by interrupted and rapidly recurring impulses, and not continuous.

For, continuous forces produce their first smallest possible effect either instantaneously or not. If instantaneously, then, the second smallest possible effect must also be instantaneous, and so the third, fourth, etc. But since time cannot be conceived as composed of instants, and since a continuous force acts, by supposition, in time,—therefore, we are compelled to assume a small interval of time to separate one instant from the other, that is to say, that the action of the force is not continuous, but interrupted and impulsive. On the other hand, if a continuous force be supposed to produce its first smallest possible effect not instantaneously, then, must a small interval of time be allowed between the commencement of the exertion of the force and the appearance of first least possible effect; and so also between the first and the second, and between the second and third, and so on. That is to say, that, on this supposition, the exertion of the force is continuous, but the taking place of the effects is interrupted.

On both suppositions, then, the action or the increase of effect will not be continuous, which proves our assertion. (See Prop. 2, Sect. III, Book II.)

The reader, who will take the trouble to concentrate his mind and think properly on the subject, will, I am convinced, see clearly for himself, that a strictly continuous effective action is an impossibility, and involves an ambiguity of language.

8. To the other various ways in which forces are usually classified, I desire, for the purpose of this treatise, to add the following two:—

First, (*a*) Transferable and (*b*) non-transferable forces. To the class of transferable forces belong heat, light electricity, etc.—all of which may be transferred from one body to another.

To the other non-transferable class of forces belong chemical affinity, cohesion, the force of crystalization, etc., all such forces that cannot be transferred from one body to another.

The force of all forces, namely, universal gravitation, occupies a unique position in this classification, as it must be looked upon as belonging to both of these classes. It is, in one sense, transferable in as much as its effects are produced on other bodies than the one which is the cause of these effects; and, in another sense, it is also non-transferable in as much as attraction proper is concerned, since the *attraction* exercised by one body upon another is not transferred to that other.

Second (*a*) Exhaustible and (*b*) Non-exhaustible forces.

By exhaustible forces, we mean such that, in producing effects on other bodies than that in which they have their seat, are, in proportion to their activity, exhausted from that body. For example, a hot body in warming another body is itself cooled in proportion as it warms the other. The force "heat" is exhaustible, and so are all the forces of nature, except universal gravitation which is non-exhaustible. The gravitating force of the sun, for example, cannot be conceived to have suffered any diminution by the effects that it has been producing from eternity on the planets.

9. It is thus seen, that the force of gravitation is peculiar in this respect, that it produces effects without suffering diminution; the motion produced by gravitation does not diminish the atractive force of the gravitating body. This fact is of paramount importance for the purpose of this treatise, and should be intelligently and clearly understood. When a stone is thrown upward, though it is positively through the exertion of terrestial gravity that it is brought back to the ground, still you cannot imagine that the attractive force of the earth has suffered any diminution in gravity through this exertion. At present, I do not mean to assert that the force with which the body strikes the ground is the *effect* of gravity, and does not represent the original impulse given to the body in its upward whirl. I will grant that the force of the original impulse became latent, (as the physicists say), during the upward journey of the projected body, and then became active again during its downward journey. But the change from kinetic to potential force, or energy, and from potential to kinetic again, has, admittedly, been caused by gravitation, without loss of gravitating force on the part of the earth.

Now, because gravitation is inexhaustible, therefore, the number of effects that may be produced by a gravitating body on other bodies is infinite; and, in this sense, may gravitation be called an infinite force in contradistinction to all other exhaustible forces, which may be termed finite, because the number and magnitude of the effects that may be produced by a body charged with electricity, for instance, is finite, and so also with other exhaustible forces, as light, heat, magnetism, chemism, etc.

The subject of this article will receive a thorough discussion and consideration in a subsequent place. In the meantime I must caution the reader not to judge too hastily, or rather misjudge the statements made here which, I confess, must at the first sight appear rather peculiar and out of all harmony with the notions entertained at present of force and energy.

10. Another peculiarity of gravitation, as distinguished from other limited and exhaustible forces, is this: with the latter, the law of physics is, that the effects of a given force is inversely proportional to the masses acted upon; while with the former, the element of mass in the bodies acted upon has no influence whatsoever; the effect being the same whatever the mass acted upon.

A quantity of coal capable of yielding, on being burned, one hundred calories of heat, will raise the temperature of *one* pound of water by 100° F; but the temperature of *two* pounds of water will be raised by the same quantity of coal by 50° F, only; and so with other forces.

With gravitation the case is altogether different. Galileo has proved centuries ago, that "heavy" bodies fall to the earth with the same accelerating velocities as "light" bodies. The moon, in her orbit, would, other things being equal, fall to the earth the same distance in a second whether her size be what it is at present or as small as an apple.

When a comet, one of those "strange bodies" makes its appearance within the solar system, thereby increasing the amount of matter acted upon by solar gravitation,—it does not in the least diminish the effect of solar gravitation on the planets.

In general, the law of gravitation as enunciated by Sir Isaac Newton is that:—

"Every particle of matter within the universe attracts *every other* particle with a force proportional directly to the product of the numbers representing their masses, and inversely to the square of the distances separating one from the other."

Now, representing the mass of the acting body by (M), that of the body acted upon by (M′), their distance by (D), the force exerted upon body (M′) will then be $(M')\left(\frac{M}{D^2}\right)$ that is for every unit mass of the body (M′) the force exerted upon it by body (M) will be $\left(\frac{M}{D^2}\right)$ no matter how large or small (M′) may be. What was to be proved.

11. Another peculiarity of the force of gravitation is, that a body cannot be said to have at one time more and at another time less of it,—the law being that the gravitation exerted by a body is, for a given distance, proportional to the mass, and mass of course is constant.

This peculiarity is important, because it at once shows the difference of the nature of gravitation from that of natural magnetism, which apparently resembles gravitation; for natural magnetism is far from being constant in its intensity, as is well known.

Indeed, natural magnetism cannot be classed as an inexhaustible force; for, being an induced state, every exertion of magnetism, undoubtedly, involves an expenditure of the energy inducing it. Natural magnetism is belived to be induced by electric currents in the body of the earth, and, therefore, every exertion of magnetism is attended by an expenditure of electric energy; and, therefore, is natural magnetism not an inexhaustible force.

So, then, gravitation stands alone, and is the only one *inexhaustible, infinite,* and *constant* force in nature. (For the most striking and most important peculiarity of gravitation see Scholium 2 and Remark to Prop. 8, Sect. III, Book II.)

12. *The Measure of Force.* Since all we know about forces are their effects, therefore, forces, if they, are measureable at all, are so through their effects; and, since their effects are (Art. 4) constituted by motions molar or molcular, and since molecular motions are in many cases difficult to be measured, and sometimes incapable of measurement altogether,—it comes to pass, that the measure of force is, generally, given in all scientific discussions in terms of molar motion; and, where the effects of a force are molecular, Science endeavors, and has partly succeeded, to find the quantitative relation between molar and the molecular motion in question, in order to be able to express the measure of molecular force in terms of molar motion. The determination of this correlation, or the finding of the molar ‘equivalent,” corresponding to certain amounts of the different molecular forces

of nature, should be one of the highest aims of physical science. Much has been done in this direction; and the determination of the "thermal equivalent," by Myer and Jule, has created a new epoch in the history of physical science. But much more is yet to be achieved in this line of research.

13. Now, since a body once set in motion by any force will, by the first of Newton's laws of motion, continue to move forever,—therefore, the whole of actual space passed over by the body could not be taken as a measure of the force that produces the motion, without limiting the space to that passed over in a unit of time. This, the space passed over by a body in a unit of time, or more correctly, the capacity of the body to pass over a certain space in a unit of time, is termed its velocity.

Now, considering velocity as a *simple* (not compound) effect of force, and bearing in mind the axiomatic truth, that simple effects must be proportional to their causes, the conclusion is inevitable, *that, for the same mass, the velocities produced by force are proportional to the amounts of force impressed upon it; and, as a corollary of this, for different masses, the amounts of force impressed are proportional to the momenta generated by those amounts.*

14. This last statement, in fact, embodies the second of Newton's laws of motions, viz.:

"All motion or change of motion in a body is proportional to the force impressed, and is in the direction of the force" (Smith's Mechanics, Harper Bros., 1865); or, as differently worded by another authority:

"Change of motion is proportional to the *moving force* impressed, and takes place in the straight line in which the force is impressed." (Newton's principia by Evans, London. Bell and Doldy, 1871.) The term "moving force" in this definition is equivalent to "momentum."

Lord Kelvin, who closely follows Newton, says: "*The measure of a force* is the quantity of motion, which it produces in a unit of time."

15. The quantitative measurement of force by momentum, as given above, since it has reference only to the amount of force impressed upon a body in producing motion, cannot be taken as a measure of the acting force in its entirety, unless that acting force be supposed to have been completely spent in producing the motion. This happens with what is called an impulsive force. We, acordingly, find in all books on mechanics and physics, something like the following statement:

(*a*) "An impulsive force is measured by the momentum it can produce in any mass." (Smith's Mechanics, Art. 233.)

(*b*) "An impulsive force is measured by the momentum it produces or destroys." (Olmsted's College Philosophy, Art. 12.)

As it will be of great advantage to the purpose of this volume, I will quote Olmsted's entire paragraph 12 in this connection:

"In order to measure such an intangible thing as force, we must look to the effects which it has, and measure them. In the case of an impulsive force it is easy, for it gives to the object acted upon a uniform velocity. Both the body and the velocity must be considered. Experiments with the same force but different bodies show that the greater the mass * * * the less the velocity which will be imparted to it. Furthermore, it will be found that with the same force the product of mass and velocity will be constant."

16. From what has been said in the previous article, it is evident that, as a measure of the quantity of the acting force, the measure by momentum is inapplicable to inexhaustible forces, that is to gravitation (see Art. 8); and *this* for the simple reason that such a force must be conceived as infinite, with respect to its possible effect (see Art. 9), and an infinite cannot be measured quantitatively.

The only possible measure for such a force, is a qualitative measure: the measure of its intensity. And this is measured by the velocity generated by the force in a unit of time, termed *acceleration.* Some works on physics give the measure of what they call a constant force by the amount of momentum generated in a unit of time; but momentum has reference to mass as well as to velocity; and, since, as has been proven in Art. 10, the element of mass has no influence on the effect, in the case of gravitation, therefore, it would seem to me that the measure by simple "acceleration" is to be preferred to that of momentum, generated in a unit of time.

17. Summary—All we know about forces are their effects. The general attribute of all effects is change. It is impossible to conceive changes in the physical world without motion, molar or molecular. Physical force, therefore, is known only as the efficient cause of molar or molecular motion.

There are two kinds of forces, finite and infinite; the measure of the one, through its effects, is the momentum it produces; the qualitative measure of intensity of the other is by the acceleration it generates in a unit of time.

BOOK I.—FORCE AND ENERGY.

SECTION II.—ENERGY.

1. In common language, "energy" is used to denote causes of change in the realm of life, corresponding to the term *force* in the physical world. So, for instance, we say, "The man is full of energy," meaning thereby that he is full of life-activity. And it would be incorrect to apply the term energy to the physical world in common language. If you should happen to say of a machine that "it is full of energy" your expression would, probably, be wondered at, if your listener be not conversant with physical terminology.

2. But, of late, the term "energy" has been forced to do service in the domain of physics, and is, generally, applied to the principle of activity a body possesses. Herbert Spencer (1st principle, Ch. vi., Part 2) applies it to all dynamical forces, as distinguished from static forces, and so makes the term "energy" almost synonymous with force. Other writers, among whom may be mentioned the great physicist John Tyndal, also used the two terms indifferently. (See first lectures of Fragments of Science.)

But in its scientific meaning, and with respect to its measure, there is the widest possible difference between force and energy.

3. While force is defined as *that which causes or tends to cause motion.* (Sec. 1, Art. 4), energy is defined by all writers as:

That (capacity) through which a body is capable of performing work:

(*a*) "*Energy is the capacity to do work*" (Olmsted's College Philosophy.)

(*b*) "*Energy*—inherit power to perform work." (Rodwell's Dictionary of Science.)

(*c*) "The fact that any *agent* is capable of doing work is usually expressed by saying that it possesses "energy." (Gannot's Physics.)

We see, from all these definitions, that the meaning of energy is made to depend, not on any definite physical entity, like *motion, effect,* or the like, but on a wholly indefinite conception—*work.*

4. Of work, like of energy, it is to be remarked that, in common language, it is used only with reference to animated things; and, in meaning, it corresponds to *effect, action,* etc., in the physical world; literally, "to work" means to exert vital force or energy. The noun *work* means an "effect" brought about by exertion of vital force, and to do work means to bring about an effect through the exertion of vital force.

The man works; the horse works; this is my work; I am working—these expressions are well known and are proper. But, it seems to me, it would sound almost absurdly to say, in common parlance, of a stone, for example, that it, the stone, works, or that it has performed any work whatsoever, unless it be metaphorically understood.

To machinery the term "work" is sometimes applied even in common language; but this is simply because, on account of unity of purpose of parts and precision in execution of machinery, it is comparable in the public mind to a living being.

5. Since by force (or energy) we mean that which produces change (Art. 2, Sect. 1), and since change, in the domain of life, is conceivable without motion, as changes in the state of consciousness, sensation, cognition, and volition, it follows, that "work," as understood in common language, does not, necessarily, involve the idea of motion, molar or molecular; and, accordingly, we apply the term "work" to mental, volitional and sensational exercises; and we say, for instance, of a student when studying much, "He is working hard;" of a piano teacher, after having listened to a lesson in music, "He has done his *work.*"

From this we see, that while with force, as applied to the physical world, *effects* are necessarily motions, the effects of energy—work—as understood in common language, are not, necessarily, motions.

6. But Science, of late, has forced the term *work* to do service in the domain of physics, precisely as it has done with its correlative term energy, and, for this purpose, has given it a definite meaning.

The best exposition on work and energy that I am acquainted with is that of A. P. Gage, in his Principles of Physics, which I will therefore quote:

"Work—Whenever a force causes a change of motion or maintains motion against resistance, it is said to do *work* * * * * * * "

"A force, to do work, must effect a change of position. Force and space are essentials of work. Force without motion is not work; motion without force is not work. The planets move but don't work. *An unbalanced force always does work.* The body that moves another body is said to do work upon it, and the body moved is said to have work done upon it * * * " (Art. 70.)

7. *Measure of Work and Energy.* "Force and space, being essentials of work ,are, necessarily, the quantities employed in calculating work. A given force, acting through a space of one foot, does a certain amount of work; it is evident that the same force, acting through a space of two feet, would do twice as much work. Hence the general formula:

$$\text{Work} = W = fs\text{''} \quad (1)$$

(f) in the above formula represents the force and (s) the space.

(*Principles of Physics.* A. P. Gage. Art. 76.)

8. Since (s) or the space passed by a body under the action of a constant force (f) is equal to $\frac{ft^2}{2}$, (t) being the time of action, therefore

$$W = fs = f.\frac{ft^2}{2} = \frac{f^2t^2}{2}; \text{ or } W = \frac{f^2t^2}{2} \quad (2)$$

and since (ft) is equal to the final velocity acquired or lost by the body, $ft = V$; therefore,

$$W = \frac{f^2t^2}{2} = \frac{V^2}{2}, \text{ or } W = \frac{V^2}{2} \quad (3)$$

that is to say, that work and energy are measured, for a unit of mass, by one-half of the square of the velocity; and, consequently, for a mass (M) by the same, multiplied by (M) or

$$W = \frac{MV^2}{2}. \quad (4)$$

9. *Varieties of Energy.* Although space has been considered to be such an essential element in the idea of work (and energy), as to constitute a factor in the product expressing its measure, still, by the common consent of physicists, the terms "work and energy" have been, indifferently, applied to molecular activities and molecular effects, such as heat, light, electricity, etc. They are all termed "energies" and "works," although the element of space is not conspicuous in them when they are effects.

"Work is said to be done when a body at a low temperature is raised to a higher temperature, just as much as when a weight is raised from a lower level to a higher level; or again, work is done when any electrical, magnetic or chemical change is produced. This extension of the meaning of the term 'work' involves a similar extension of the meaning of the term 'energy,' which, in this wider sense, may be defined as the *capacity of producing physical change.*" (Gannot's Physics, Art. 63.)

It is to be remarked, that this wider definition of energy corresponds exactly to the definition of force, as given in Art. 3, Sect. 1. Consequently, we have as many varieties of energy as there are forces in nature. But, broadly speaking, energy is spoken of as: (1) Molar or (2) molecular.

10. Of molar energy, physicists distinguish two varieties: (*a*) Kinetic. (*b*) Potential.

(*a*) Kinetic energy is *"the capacity for doing work possessed by a mass in virtue of its motion."* (Gage's Principle of Physics, Art. 72.)

(*b*) Potential energy is *"the capacity for doing work possessed by a body in virtue of its position, being such that it is possible for it to move, and in virtue of the existence of a stress which tends to move it."* (Same Art. in Gage's.)

11. *Examples of Potential Energy.* (*a*) A bent bow, or a watch spring newly wound up, are said to possess potential energy, because in virtue of the position of their particles with respect to one another, and because of the stress of the same particles to return to their normal position—they, the spring or bow, are capable, on being released from obstacles, to produce motion.

(*b*) A quantity of carbon, for instance, is also said to possess potential energy, because, in virtue of its position with respect to oxygen, and because of chemical affinity; that is, its stress or tendency to unite with oxygen—it, the carbon, is capable, on being united with oxygen, to produce molecular motion—heat.

(*c*) The most striking example of potential energy is that of a body projected vertically upward against the force of gravity. In its upward ascent it continually loses its kinetic energy, which becomes nil when it reaches the highest point of its ascent. But at this point it possesses potential energy in virtue of its position with respect to the earth, and the stress that the force of gravity exercises on it.

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12. Because actual energy, in many cases, produces potential energy, and vice versa; and because of the doctrine of conservation of energy,—physicists have been constrained to regard them as mutually representing each other. So they say, for instance, of a body projected upward, when it reaches its highest point, that its kinetic energy, although apparently lost, has in reality not been absolutely destroyed, but has been converted into potential energy.

13. If we carefully and intelligently analyze the three examples of potential energy, as given above, we cannot fail to notice the wide difference that there is between the first two and the third; and it is this: In the first two the force of the stress, by virtue of which the bodies tend to assume their original position, had no existence before the condition of potentiality was obtained through the expenditure of actual energy; so that you may fairly well say that the actual energy spent is represented by that stress; and, moreover, that stress is in the body itself, and when it, the body, does assume its original position, and realizes, again, the original actual energy, it does so, without the expenditure on it of any energy from without. So, for instance, in the case of the bent bow, the actual energy spent in bending it is fairly well represented by the amount of the elastic strain in the bow, and when it did unbend and actual energy again was realized, it did so solely by virtue of that elasticity without having had any energy from without spent on it.

The same reasoning holds good in the second example: The actual energy spent by the sun in separating the carbon from the oxygen is fairly well represented by the force—chemical affinity—which did not exist in the carbon while in combination with oxygen in the molecule of CO_2; and, when on combining again with oxygen actual energy is again realized, it is so realized by virtue of the chemical affinity in the atoms of carbon, without having any energy spent from without.

But the case is altogether different in the third example: When the body reaches the highest point, it possesses no new force which shall represent the kinetic energy lost; since the stress by which it tends to return to the earth is due, not to the raised body itself, but to the constant gravitating force of the earth; and, moreover, when it does return to the earth, and actual energy is realized, is it not because *work* has been done *on it* in its descent by the earth? If the body at its highest point possesses any *capacity* at all, it is clearly, not the capacity for *doing* work, but a capacity for having work *done upon it* by gravity. And is it not a contradiction of terms to call this *energy,* which is defined as capacity for *doing* work? You might as well say of a discharged Leyden jar that it possesses potential energy, since it possesses the capacity of being recharged.

14. As the above reasoning, with respect to the distinction between the first two and the third examples, will not, perhaps, stands a rigorous criticism, I will add the following:

In the case of the bent bow, the intensity of the elastic pull or strain is proportional to the amount of actual energy spent for having the bow bent. The more you bend the bow and the more of actual energy you spend for having it bent, the more vigorous is the elastic pull developed, and the greater the amount of resistance necessary to restrain the bow from unbend-ing. You may, therefore, with a fair show of reason, say that the "stress," the elasticity, represents the actual energy spent—there being a proportionality between the two.

In the case of the carbon, also, the amount of actual energy required for separating a given quantity of carbon from oxygen is constant; and so is the intensity of the tendency of the carbon to unite with oxygen—chemical affinity—also constant. Therefore; you may say that the latter represents the former.

With a body projected upward against gravity the case is entirely different. While the actual energy spent for having the body raised may be little or much the stress between the body and the earth, that is to say, gravity, is constant—they, therefore, bear no relation to each other. A body raised a hundred feet from the ground is under the same "stress" as a body raised a thousand feet. The stress between the body and the earth—gravity—can, therefore, not be taken as representing the actual energy spent.

The only thing, then, gained by the body through the expenditure of actual energy in raising it, is only an "advantage of position;" and can this advantage of position be said to represent the actual energy?

This "advantage of position" is, indeed, of great advantage to *us*, living beings; and, as far as *our interests* are concerned, we may well call it *energy in store;* since whenever a body is in that "advantage position" we are certain to get energy and to be able to utilize it with little or no exertion on our part at all, the exertion being done for us by the force of nature—gravitation. Moreover, it is of advantage to us also, because it renders possible *for us* the accumulation of small efforts on our part for the production of comparatively great effects. The smith, for instance, wishing to thin a piece of metal, could not very well do it directly by his effort—say, by squeezing it with his hands. What does he do, then? He raises his hammer, gradually, inch by inch, and places it in an "advantageous position" and then lets it fall on the metal and gets back the energy he spent on raising the hammer, but he gets it in one lump, so to say.

The terms "latent energy," "potential energy" and "energy of position" are also all very useful in science as indicating, first, the disappearance of actual energy in producing displacement, and secondly, the readiness of having work done on the displaced body by the natural forces—gravity.

This is all very true and proper; but to say, in a strictly physical sense, that this nothingness of position represents that "something," which we conceive as the attribute of a body possessing actual energy, seems to me to be entirely out of all accord with reason. That motion, which is a property of a body, and can be manifested by a body only, should be converted into "position," which is only a relation of space, and is, moreover, only a psychological conception and not an existence, is most inconceivable.

True, physicists have been careful in framing the definition of energy to exclude all allusion to a "something existing." It is a mere *capacity* for doing work, they say. But reason utterly refuses to attribute to matter mere abstract capacity, without having it tacitly understood that there is a something "to which that capacity is due." Herbert Spencer expresses himself thus with reference to this conversion of actual into positional energy:

"We have to conceive that a *power* is changed into a space relation, and this is inconceivable." (1st Principles, pp. 11, Ch. v.)

He tries to solve the difficulty by supposing that the original motion is converted into a kind of strain in the medium pervading space between the earth and the ascending body, just the same as an elastic cord is strained by motion. This seems to me to be an unwarrantable assumption at best; and besides, if there be a strain, it ought to be exerted on the raised body besides, and in addition to gravitation, which we know it does not. For you cannot imagine the force of gravitation ever to cease, since the lunar deflection from a right line never ceases.

Professor Tyndal, in his *Fragments of Science,* also attempts a solution of the difficulty by another but similarly skillful hypothesis. Professor Ames, in his *Theory of Physics,* says, in effect, that no idea can be formed at present of what the nature of this potential energy is. "Ultimately, of course," he says, "it may be possible to describe any form of potential energy in terms of motion of matter, when the phenomenon comes to be better understood. So *potential energy* must be understood to be a term which is used to cover our present ignorance." (Theory of Physics, Art. 68.)

15. All these strained and purely hypothetical suppositions and admissions of ignorance are rendered necessary because of the doctrine of conservation of energy. Without this doctrine there would be no difficulty at all, because, in that case, the disappearance of motion in the ascending body would be looked upon (as it was looked upon by Newton) as simply lost or destroyed through the force of gravity acting in a

contrary way to the original motion; and the advantage of position, also, would be looked upon, merely, as a circumstance consequential upon the original motion, but not as anything essential, nor as representing any force, motion, or energy at all.

Let us, therefore, examine this doctrine of conservation of energy. What does it affirm, and what are the inductions, if any, upon which it is based, and, lastly, whether its deductions are warranted by those inductions?

16. *The Law of Conservation of Energy.*

(*a*) "In any system of bodies whatever, to which no energy is communicated by external bodies, and which parts with no energy to external bodies, the sum of the various potential and kinetic energies remains forever unaltered. (Unseen Universe, Art. 102.)

(*b*) "The total energy of any body or system of bodies is a quantity which can neither be increased nor diminished by any mutual action of these bodies, though it may be transformed into any of the forms of which energy is susceptible." (Maxwell, quoted by Gage's Principles of Physics.)

Of course, Maxwell tacitly assumes that *advantage of position* is a "form of which energy is susceptible," as otherwise the law would not hold good.

Gage follows Maxwell statement of the law by the following:

"Since all bodies of matter in the universe constitute a system, it follows from the above that the sum total of energy in the universe is a constant quantity. Neither creation nor annihilation of energy is possible through any agency known to man." (Art. 264, Principles of Physics.)

17. The first question that presents itself to the mind in regard to this law is this: Is this law an apriori necessary truth, or is it an aposteriori deductive truth—that is, a generalized truth derived from experiences or experiments?

That the denial of this law does not involve a logical contradiction nor any negation of the evidence of consciousness; that is to say, that it is not an apriori truth, is proved by the fact that the opinion of the bulk of mankind, from prehistoric times up to the present, is in opposition to this law.

Herbert Spencer says that it "is by no means self-evident to primitive men or to the uncultured among ourselves; contrariwise to undeveloped minds, the opposite seems self-evident. The fact that a stone thrown up soon loses its ascending motion, and that after the blow its fall gives to the earth it remains quiescent, apparently proves that the principal of activity which the stone manifested may disappear absolutely." (1st Principles, Part II., Ch. V.)

Indeed, it is not very evident how this law could ever have been dreamt of, even by the cultured, prior to the days of Count Rumford, Davy, Myer, Jule, and Grove, when heat was considered, not a molecular motion, but a substance.

It is claimed, however, that Newton laid the first foundation to this principle of conservation. He is made to say, in his interpretation to the third law of motion, something equivalent to the following statement:

"Work done on any system of bodies has its equivalent in work done against friction, molecular forces or gravity, if there be no aceleration; but if there be acceleration part of the work is expended in overcoming the resistance to aceleration, and the additional kinetic energy developed is equivalent to the work so spent." (See Unseen Universe, Art. 69. Also Rodwell's Dictionary of Science.)

I fail to see any allusion to conservation in this statement. All that may legitimately be inferred from this passage is (it seems to me) that a certain definite amount of energy is required to overcome a certain definite amount of friction, molecular forces, or gravity; but that the energy so spent, after it has done its work against "friction, molecular forces, or gravity" should be conserved, is far from being evident from this statement.

I have before me an old English edition of the Principia, published in 1803, and I find the original statement above referred to in the scholium to the laws. I will copy it verbatim: "For if machines are so contrived that the velocities of the agent and resistant are reciprocally as their forces (masses), the agent will just sustain the resistant, but with a greater disparity of velocity will overcome it. So that if the disparity of velocities is so great as to overcome all that resistance, which commonly arises either from the attrition of bodies as they slide by one another, or from the cohesion of continuous bodies that are to be separated, or from the weights of bodies to be raised, the excess of the force remaining after all those resistances are overcome, will produce an acceleration of motion proportional thereto, as well in the parts of the machine as in the resisting body."

This passage is, probably, the original from which the above is a transformed statement. Now, what does Newton say here? Merely this: That the excess of force after the different resistances are overcome will produce an acceleration proportional to that excess. Can you infer any conservation from this? And is not, rather, the contrary of conservation more legitimately inferable from it? Only the excess produces an acceleration, but the other part of force is spent (lost) in overcoming resistance.

In the same scholium Newton expressly says; "When a body is thrown upward its uniform gravity impresses forces and *takes off velocities* proportional to the times."

Again, in the beginning of the same scholium, while discussing the possible effects of a collision between two bodies, one having ten parts of motion and the other six parts of motion, he says, in effect, that if the last body gain by the collision twelve parts of motion, it will proceed, of course, with eighteen parts of motion, while the first body, having lost two parts of motion more than it originally had, will proceed with two parts of motion in a cotnrary direction. So that 18 — 2 = 16. Now, it is plain that if Newton should have had the slightest belief in conservation, he would have never said it, even as an example; since the sum of the absolute quantities of the two motions, regardless of direction, is equal to 20, that is virtually a creation of four parts of motion.

18. Enough has been said to show that the denial of this law of conservation is not in itself contrary to the elements of reason. It must, then, be an inductive truth—that is, a truth derived from experiences and experiments, which it really is, as will be gathered, I hope, from what follows.

Davy has succeeded in liquifying ice, by friction in a vacuum, thus showing that the energy consumed in overcoming the resistance that (in the language of Newton) arises from "the attrition of bodies as they slide by one another," is not, in overcoming the resistance, absolutely lost, but actually reappears as *heat*, the heat, thus generated, liquifying the ice. Count Rumford has, likewise, succeeded in raising the temperature of water to the boiling point by the boring of cannon, thus proving that the energy spent in overcoming the resistance arising "from the cohesion of continuous bodies that are to be separated," is not, in overcoming this resistance, destroyed, but again reappears as heat. Moreover, Myer and Jule have succeeded, even, in establishing a quantitative relation between the amount of kinetic energy employed in overcoming these two kinds of resistance and the amount of heat generated by them.

Many and varied experiments have, since the days of those great pioneering men, been performed by prominent and skillful students of nature, all of them invariably proving that when any form of energy is employed in overcoming either one of these resistances —that is to say, resistances due to friction or to molecular forces; the amount of energy thus employed was not lost in overcoming the resistance, but always reappeared either in its original form or in some other form of energy, and in amounts definitely proportioned to the quantity of original energy employed.

That the energy employed in overcoming the 4th resistance of Newton—namely, the resistance to acceleration—is, also, not lost, is self-evident, since it reappears as kinetic energy.

In general, it was experimentally proven that whenever any form of energy apparently disappeared, that in reality it was not absolutely lost but reappeared (either after having performed some work or not, as in the collision of bodies), either in its original form and quantity, or in some other form of energy, in defiproportion.

Conversely, whenever any form of energy appeared suddenly, it has always been proved to be traceable to some previous form of energy.

It was also proven by experiments that whenever energy disappeared from one body it invariably made its appearance in some other body, on which generally work has been done by the 1st body. And, conversely, whenever energy made its appearance in one body, it was invariably shown to have disappeared from some other body, which other body, generally, was shown to have done work on the 1st body.

(The dissipation of heat by radiation into space forms no exception to this statement, since this radiation is conceived to be only a transference of energy from the heated body to the ether filling space.)

The results of all these multiform and varied, experiments positively point to a conservation of energy; there can be no reasonable doubt about that. Accordingly, on the strength of these experimental facts, although the range of all these experiments was, as it must of necessity be, limited in space and time and in the nature of the forces and effect sexperimented upon,—still on the strength of these facts, and because of the law of the uniformity of nature, the generalized and extended law of conservation of energy was formulated.

One fact, constituting apparently a contradiction to this general law, had to be somehow explained before this law could be accepted as valid; and this is the following:—

When energy was spent in overcoming the third resistance of Newton, namely, resistance arising "from the weight of bodies to be raised"—it, the energy so employed, was, apparently, absolutely lost; experiments could not discover any form of *actual* energy, either in the body raised or in any other body, to represent the original energy spent in raising the body. How could this fact be brought under the general law of conservation?

But human ingenuity seldom fails in its resources, and this difficulty was easily done away with by calling the change of position, which of necessity takes place when a body is raised, by the name "energy," "potential energy," and by defining the term "energy," not as an existence by virtue of which a body possessing it is capable of doing work, but as a mere *capacity* for doing work. (See Arts. 13 and 14.)

19. In passing, I wish to remark in addition to what was said in Arts. 13 and 14, that if you strictly adhere to the definition of energy as mere *capacity,* not as anything substantial, then the law of conservation of energy would be an open contradiction to the law of dissipation of energy; since dissipation of energy means degradation of energy, that is to say, a lessening of capacity for performing work; and if "capacity for performing work" is energy, then dissipation of energy, being a lessening of "capacity to perform work," means a lessening of energy or absolute loss of energy, contrary to the assertion of the law of conservation!

Tait says, "The *available* energy of the universe tends to zero," meaning by the term *available,* capable of doing work. The implication of this statement is, obviously, that there is present in the universe a quantity of unavailable energy. Now, "unavailable energy" is a contradiction of terms, if by *energy* you understand "capacity for doing work." (See Gage's Principles of Physics, Art. 205.)

But "dissipation of energy," being a fact, cannot be done away with; and if so, either your law of conservation must fall, or you must modify your definition of energy as meaning "something," not mere "capacity." (See Unseen Universe, Art. 104.)

In fact, capacity to perform work can at best be a temporary affair only, even in the case of molar motion.

For, any number of bodies moving with the same velocity and in the same direction cannot perform work on each other; that is to say, they possess no capacity to perform work with respect to one another. They therefore might be said, if the definition of energy as "capacity" be strictly adhered to, to possess no energy. It is only when the bodies move with different velocities or in different directions or both, so that they may and will impinge upon one another, and so perform work upon one another,—that they really possess capacity for doing work. But this condition of affairs is, evidently, but temporary, as anyone can see for himself.

20. After this slight digression, we will return to our subject, the law of conservation of energy, and in the following pages we will apply ourselves to examine whether the deductions of the law are warranted by its inductions. But before proceeding further, I must ask the reader's indulgence and patience not to be shocked, if I happen to assert things contrary to received and sanctioned opinions: opinions, received

and sanctified by the assent of the greatest men of the 19th century.

I know, it must seem the height of folly to doubt, question, and examine, even for a moment, the validity of such a law as the law of conservation of energy; a law, upheld by such great men as Myer, Jule, and Grove, Thomson and Young, Tait and Stewart, etc., etc.—in a word, by all the great philosophers, physicists, astronomers, and mathematicians of the 19th century, without a single exception, as far as I know; a law, that has become the criterion of all philosophic and scientific statements, and any statement or opinion that infringes upon it is at once rejected as non-valid, without further examination; a law, the assumption of which is said to be "a supreme act of faith," the definite expression of a trust that the Infinite Sustainer of the Universe "will not put us to permanent intellectual confusion" (Unseen World); a law, of which Stallo expresses himself thus:—"When all the phenomenal changes in the universe shall have been reduced to the one principle of the conservation of energy, the time will have come for celebrating the final consummation of physical science in a new epic 'de rerum natura'!" (Modern Physics.)

That such a law should be examined as to its validity must, indeed, appear either as folly, or as arrogance, and want of respect. What apology shall I or can I offer? I can only assure and reassure a hundred and a thousand times over and over again that it is not out of vain arrogance and want of respect to authority that I go to search for weak points in this grand law; but it is, in truth and with all sincerity, out of the deepest possible conviction, that this law, generalized and extended as it is, embracing, as it does, all forces and all kinds of energy in the infinite universe, without exception—that this law, I wish to say, does infringe upon the greatest of all authority,—Truth. Truth, before which all authority, even from the prehistoric times up to the present, should be considered as naught. Truth, the greatest criterion of all criteria. Truth! Fact! the final word of *God* spoken through nature!

21. It was stated in Art. 18, that "the results of all these multiform and varied experiments *point* to a conservation of energy," meaning by *a conservation,* some sort of conservation. But whether this conservation should be general, I mean predicable of *all* and every variety and kind of energy in the universe, or limited,—that is to say, predicable only of some forces or energies of nature, and not of others—this is an open question, the answer to which is manifestly dependent on the nature and number of forces experimented upon. Because, surely, to include in the statement of the law of conservation forces or energies that have not or could not be experimented upon, would manifestly be an unwarranted assumption; especially, if the forces, not experimented upon, should happen to be widely differently in nature from those experimented upon. You might say: 'Is not the *correlation of forces* a fact? Has not Grove and many after him conclusively proven it?' If so, what is true of one kind of force or energy must be and is true of every and all kinds of force or energy. Granted. But what if there be a force of which neither Grove nor any one else affirms nor could affirm any correlation with other forces? Such a force, that other forms of energy cannot, even, be conceived to be convertible into it, nor is it, in a proper sense, convertible into any other forms of force or energy? And, if this force or form of

energy, which is not in any correlation with other forms of energy, be not among the forms of energy experimented upon in reference to the law of conservation of energy—would, do you think, under such circumstances, a generalized statement of the law of conservation, predicating conservation of energy in the *universe,* and making the sum of all the different forms of energy a constant quantity; would such a statement, do you think, be justified under the above supposition? Assuredly not. Since that force might, (for all we know), in its action upon matter, cause either an increase or diminution of the absolute quantity of energy in the universe. Is this not clear?

But, there is such a force or form of energy in the universe; and that is the force, which in Art. 7 of Sect. I. I termed "the force of all forces," and in Art. 8 of the same section I termed "infinite"—a force that is considered by some, at least, to be a primary attribute of matter, and is classed with extension and resistance—in a word, it is the force that binds worlds together, and renders the order of the universe, as it exists, possible. It is—UNIVERSAL GRAVITATION.

22. That the force of universal gravitation, which may and must be considered as a form of energy, since it undoubtedly produces "acceleration," and by definition everything that produces "acceleration" is to be termed energy (See Art. 6 of this Sect.)—that this form of energy is widely different in its nature from all other forms of energy, in many peculiarities, may be gathered from what was said in Arts. 7, 8, 9 of the 1st Section, where, among other peculiarities, we have pointed out one of great importance for us, namely, that while all other forces of nature are limited and exhaustible this, universal gravitation, is

unlimited and inexhaustible. Now, on carefully analyzing the experiments performed and the facts brought forward in establishing both the doctrine of correlation and the doctrine of conservation, it will be found that the forces involved in those experiments and facts, all of them, invariably, belong to the first class only, that is to say, to the limited and exhaustible forces, only. So that universal gravitation, which is an unlimited and an inexhaustible force, was entirely left out of consideration in establishing both of these doctrines.

First, as to correlation. I have before me "The Correlation of Physical Forces," by Grove, the father of the theory of correlation; and I find that he devotes a separate section of the book to each of the following physical forces: motion, heat, electricity, light, magnetism, and chemical affinity, where he discusses and proves the correlation of each of the above forces to the remaining of the above forces; excepting universal gravitation, on which he hardly touches in the entire book, and of which he says, in passing, while treating of other modes of force: "Gravitation, inertia, and aggregation were but cursorily alluded to in my original lectures; their relation to other modes of force seemed to be *less definitely* traceable; but the phenomenal effects of gravitation and inertia, being motion and resistance to motion, in considering motion, I have in some degree included their relations to other forces."

Now, a little reflection, I think, may convince us that not only is the correlation of universal gravitation to the other forces *less definitely traceable,* but that hardly *any trace* is detectable. The only connecting link between universal gravitation and all other forces, is, as far as we know, that it produces motion. But is this motion a continuation of the original force, gravitation, that produced the motion, as is the case with other correlations? Assuredly not; for, if yes, then the intensity of gravitation ought to get diminished whenever it had produced an acceleration; and as a consequence of this, the gravitating force of the sun, which continually produces new motions in the revolving planets, would continually go on diminishing, which is cantrary to fact.

"I venture," says Grove, "to regard the heat which results from friction or percussion as a *continuation of the force* which was previously associated with the moving body, and which, when this impinges on another body, *ceasing* to exist as gross palpable motion, *continues* to exist as heat." (Correlation of the Physical Forces.)

Can you affirm, I ask, a correlation in the same sense between gravitation and the acceleration, or motion produced by it? It is, perhaps, superfluous to add that neither does gravitation *directly* produce any other form of force, excepting motion, nor do the forces, light, heat, motion, electricity, magnetism, ever produce gravitation, whether directly or indirectly.

23. What I meant by saying that the sun "continually produces *new motions* in the revolving planets," may need some explanation. Imagine the gravitating power of the sun to cease, then all the planets would, by inertia, move away into the infinite space in straight lines, tangental to their orbits at the point where they might have happened to be when the gravitating force ceased. Imagine, again, that while the gravitating force continues, this inertial tangental motion suddenly destroyed, then, all the planets would, by gravitation, move in straight lines to the centre or sun. Now, it is by the continual composition of these

two motions—the old inertial tangental motion and the continually renewed central or radial motion—it is by the composition of these, that the actual motion of the planets, in their orbits, is effected. (See Newton Principia, Book I, Sect. II, Propositions 1, 2.) Now, it is to this nascent, continually renewed, radial motion that I referred, when I said that the sun, "continually produces *new* motions in the revolving planets."

24. Enough has been said, I think, to show that the correlation of gravitation to other forms of energy is far from having been proved. Indeed, apriorily, we should be inclined to say, that a force, whose intensity is under all possible conditions unalterable, can not be said to have any correlation with other force, in the strict sense in which the term "correlation" is physically understood, and the phrase "correlation of gravitation to other forces" is really a contradiction of terms. We will now proceed to examine the relation of gravitation to the law of conservation.

25. All experimental proofs and observable facts upon which the generalized law of conservation is based, will, on close analysis, be found to belong, exclusively, to the class of forces termed by me *exhaustible forces*: motion, heat, light, electricity, chemical affinity. As for universal gravitation, not only is there a positive absence of any experimental proof to show its subjection to the law of conservation, but the facts presented spontaneously to us by nature are, apparently, at least, in open contradiction to the law. The kinetic energy of a stone thrown upwards is apparently positively and absolutely destroyed by gravitation,

when the stone reaches the highest point of its journey; and, conversely, when descending, the stone gains kinetic energy, this energy has, apparently, been originated de novo through the action of gravity, without there having disappeared any other energy from any place; since the intensity of gravitation is constant and cannot be conceived to have suffered any diminution, as we have proved above. These are facts spoken to us by Nature in her plain, simple, natural language; and this is, also, the way in which the bulk of humanity comprehends them. But we, scientific men, because we have observed a constancy, a conservation, in the interaction of all other forces of nature among themselves; and because of the instinctive and almost irrepressible love to generalize and deal with universals; and because these facts, uninterpreted, stand in the way to a general law of conservation—we, therefore, endeavored to interpret this simple language of nature, in a way suited to our purpose; and have easily succeeded in doing so by the aid of a single phrase "potential energy." And this potentiality does excellent service for us in the capacity of a connecting bridge between the two historically remote events: the loss and regain of energy across the vacuum of time; the energy regained on the body's descent is only the energy lost in its ascent after having passed a longer or shorter stage of potentiality. 'And what,' one may ask, 'was the function of gravitation in this series of metamorphoses? Has gravitation done anything at all?' 'Assuredly,' will probably be the rejoinder, 'it has changed the direction of the motion from an upward course to a downward course.' 'And is not force required for change of direction? Has not Newton so assured us in his first law of motion? And, if so, force has then been exerted, and work done and lost?' 'But,' will probably be the answer, 'we were arguing about energy, not about force. Force is different from energy.' 'But what is your distinction between force and energy? Besides, it is not evident why you should not term gravitation an *energy*. Is it not capable of performing *work?* Does it not cause acceleration, or does it not cause motion, even against resistance, say friction? Moreover, if you term gravity a force, and assume that force is something different from energy, then, of course, the law of conservation of energy has no reference to gravitation; and if such an active universal force as gravitation has been taken no account of in establishing your law, do you think that such a law *must* be accepted as valid?'

It is difficult to surmise what the answer to these questions should be.

26. It may be asked:—Suppose we admit that gravity does destroy the motion of the ascending body, positively and absolutely, and that it does generate motion de novo in the descending body; in other words, suppose we admit that gravitation, in general, is a force capable of originating motion and also of destroying motion, under certain conditions,—what should be our attitude towards the law of conservation? Must we reject it entirely and absolutely, or, perhaps, consider it as valid under certain limitation?

27. In answer to this, we should like to say that conservation of energy, within the range of the exhaustible forces, is, undoubtedly, a fact, as has been abundantly proven by numerous experiments. But it is, under the above supposition, only valid within the range of the exhaustible forces, exclusive of gravitation; that is to say that heat, light, electricity, motion,

etc., in their action upon matter or in their interaction among themselves can never be destroyed, without the intervention of gravitation, nor can they, without the intervention of gravitation, make their appearance in any body without having disappeared from another body. This is all positively true, only, if you exclude the intervention of gravitation. For, since we assume that gravitation does destroy and generate molar motion, and since all other forces are correlated to molar motion—it follows, that gravitation may, under certain conditions, either destroy or generate de novo all the other forces, through the intermediacy of molar motion; and thus either increase or diminish the absolute amount of energy in the universe.

28. It may be further asked: Since to render possible the generation of motion de novo by gravitation, advantage of position is a necessity; and, since in order that a body may gain advantage of position, kinetic energy must be destroyed by gravitation,—what difference, then, does it make, whether you look upon the lost motion of the raised body as absolutely destroyed and the "position" you consider as a mere circumstance; and also the motion of the descending body you consider as originated de novo;—or you consider it in the old way. that the kinetic energy is converted into potential energy, and the potential energy back again into kinetic energy? Since the sum total of the actual energy and the energy that by position is rendered possible to be generated de novo, as you say, is a constant quantity, even under your supposition?

You may be right in everything that you say, but try and substitute "possibility of energy being regenerated" for "potential energy," and, with slight modification of language, the law of conservation remains unshaken except, perhaps, for certain statements that energy cann not be destroyed nor created. Since, even according to *your* view of the matter, you cannot get energy generated de novo, unless it has previously been destroyed by gravitation, as is evident on a little reflection; similarly, you cannot get motion destroyed by gravitation, unless you get the body possessing that motion in a position which renders the regeneration of motion possible?

29. In answer to this, it is to be remarked that the assertion implied in the previous article, that you cannot get energy generated de novo by gravitation, unless it has been previously destroyed; and that whenever kinetic energy is destroyed by gravitation you, necessarily, get the body whose motion has been destroyed, in a "position of advantage,"—it is to be remarked, that this assertion is far from being a proved fact. True, in our experience with terrestrial gravity, such is generally the case; but this does not negate the possibility, that in the wide domain of the cosmos—nay, even in the domain of the solar system conditions may not obtain, which render possible the generation of energy (through the intermediacy of motion) by gravitation, without there having been any energy previously destroyed (by gravitation) in order to place the body acted upon in a "position of advantage." Once you admit the creative and annihilative power of gravitation, as far as molar motion is concerned, and such a supposition is not without the pale of possibility.

In Book IV of the 1st part of this work, and in the 2nd part of it, it will be shown and proven that not only "is it not without the pale of possibility," but that it is the most sacred and most sublime law of nature. I know that these utterances will appear mysterious and perhaps ridiculous; but I, nevertheless, could not refrain from uttering them, although perfectly aware of the danger of anticipating things.

Without, however, referring to any subsequent developments in this work, we will attempt, right here, to show that although it does seem self-evident that for a body to fall it *must* previously be raised, and if so, energy, then, must be destroyed;—that this is only seemingly so.

For, the raising of one body (B) from another body (A), attracting it, may be effected by the gravitating force of a third body (C), if, by reason of inherent motion in any or all of these bodies, the position of body (B) in its relation to body (A) and (C) is periodically changed; so that at one time, the attractive force of body (A) predominates, and at another time the attractive force of body (C) predominates on it; so that there ensues a periodical fall of body (B), alternately, either to A when *its* attractive force predominates, or to C when *its* attractive force predominates; the fall to A constituting a raising of B as relative to C, and vice versa. Now, if you suppose that the molar motion, generated in B in its falling, whether to the one or to the other body, is by some means converted into some molecular form of energy on which, as is well known, gravitation has no direct effect or influence,—then, of course, at each fall of B and on the conversion into molecular energy of the molar energy thus generated, there will occur an addition of new actual energy to the whole amount of energy then existing in the universe, without any energy having been previously destroyed, or made to disappear from any body or place.

This supposition is not merely a web of imagination, or a pure mathematical conception of the possibilities of things,—it is a *fact!* Such a concurrence of conditions does actually occur in nature, as will presently be shown.

30. The case of an exterior planet perturbing the motion of a planet interior to it; the case of Venus, for instance, affecting the motion of Mercury, is a good illustration of our meaning. When Mercury is in a certain position, with respect to the Sun and Venus, it is caused, through the action of the gravitating force of Venus, to leave its normal orbit, in a direction away from the sun; that is to say, it is raised with respect to the sun. When Mercury again, gains, through its motion, another certain position, so that the action of Venus upon it in a certain direction, either ceases or is very much diminisher—it, Mercury, falls back again nearer to the sun, and assumes its normal orbital motion or nearly so; and in its fall gains kinetic energy, of course. In a third position, Mercury is dragged out of its course nearer to the sun; and in a fourth position it, again, nearly regains its previous motion, etc. These different perturbations do not compensate or cancel each other in the course of one revolution, either of Mercury or of Venus. It takes a long period with many revolutions of both planets before such cancelling is effected. As I don't intend to enter into a minute disquisition of the subject of perturbation, for particulars see "Newton's Principia, Book I, Sect. IX, Proposition LXVI.

It is thus seen that through the action of gravitation, itself, without expenditure of any other energy, a body may be caused to be raised from another body. Now, if you imagine that after it has raised that body, the gravitating force of the perturbing body, for some reason or other, ceases, in some measure, to be effective on the raised body,—this body will then fall to that from which it has been raised and will gain its former position, and, at the same time, acquire an amount of kinetic energy which it did not possess before; and if this amount of kinetic energy should be, for some reason, converted into heat or other forms of molecular energy on which gravitation can have no direct effect—then, this molecular energy will be a permanent addition to the sum total of universal energy. True, in this last instance of Mercury and Venus, such is not the case; for the gravitating force of Venus destroys its own effects after a longer or shorter period, as we have said above. But there is such a case in nature. Such a concurrence of conditions does take place in nature. I refer to the tides.

31. Through the action of the gravitating force of the sun or moon upon the waters of the ocean or lakes and rivers, etc., the very well known tidal protuberance is produced; the summit of this protuberance is, or ought to be, on the line, joining the earth's centre with that of the sun or moon.

It is immaterial for our purpose what the exact modus operandi in the production of this "tide" actually is. All we need, is an admission that the gravitating force of the sun or moon is, and is the *only* efficient cause of its production.

Now, were it not for the rotation of the earth around its axis, a comparatively permanent "tide" would be the result; but as the earth moves around its axis with a velocity at the equator of more than 1000 miles in an hour, different portions of the surface of the water are brought under the more proximate and more efficient attraction of the sun or moon—that is, in close proximity to the line joining the earth's centre with that of the sun or moon. As a consequence of this, new tidal protuberances are continually produced, while the old ones, moving away, as they do, eastward from the place of strongest solar or lunar attraction, gradually subside, that is to say, fall nearer to the earth through the action of the earth's gravitation. In this way, the "tidal wave" seems to travel from east to west, following the apparent diurnal motion of the sun or moon. The water constituting any tidal protuberance does not move bodily from east to west; in fact, it moves with the earth in a contrary direction. It is only the upward and downward motion of the "tide" that moves.

There is, indeed, a certain horizontal bodily motion of the waters of the ocean: the *ebb* and *flow*. But this motion is very slow, compared with the motion of the tidal wave, and is consequential upon it; for the waters of the ocean rush from all sides towards the tidal protuberance. And, therefore, for any one place, the direction of the "tidal current" changes with the position of the tidal protuberance.

The case of the tides perfectly exemplifies and illustrates our example of the three bodies in the previous article. Here, a particle of water, on or near the surface, free to move only upward, since on all other sides it is hemmed in by the large mass of the water of the ocean, and by the earth beneath it,—this particle of water may be looked upon as being "body B;" the earth, attracting it to the centre, as "body A;" the sun or moon, counteracting the earth's gravitation, as "body C." Now, it is by reason of the inherent motion of this particle, which moves together with the earth around its axis, that it is gradually brought in proximity with the line joining the centre of the earth with that of the sun or moon, where the attractive force of "body C," the sun or moon, predominates on it, so to speak, and raises it from the earth; and by the same inherent motion of the particle, it is, then, carried away, from the above mentioned line, to a place where the attractive force of the sun or moon is less, and the attractive force of "body A," the earth, predominates on it, so to speak, and it falls back again to the earth, and while falling it acquires, in its fall a certain velocity, that is to say, actual kinetic energy, which is eventually arrested, either through friction, or by impact against the shore, and is so converted into heat on which gravitation can no more act directly. Here, then, we see the production of heat, that is actual energy, without expenditure of any other energy, either actual or potential, solely by the exertion of gravitation.

32. What I just now described as the career of a particle of water subjected to the action of the "tide generating force," is merely illustrative; and may, with equal force, be applied to any of the theories of the tides; for it is immaterial to us what theory of tides you adopt, whether the static or dynamic; whether the tidal protuberance ought to be on the line joining the centre of the earth with that of the sun or moon, or 90° away from it,—all this is immaterial to us. All that is esential, is an admission that the only efficient cause of the rise of the water, no matter where it may happen to be, is the gravitating force of the sun or moon alone, counteracting the gravitating force of the earth (the word "predominate" employed throughout the previous article has only a figurative meaning);

and that the fall of the waters is caused by the cessation of this counteraction of the moon's or sun's gravitation.

This much, I think, will, and must, be granted to us by every rational man who is at all acquainted with the subject, and who will give it a thorough consideration.

Once is this admitted, and it immediately follows, that the enormous amount of kinetic energy displayed by the tides is produced, so to speak, de novo, by the action of gravitation alone, without there having disappeared any form of other energy whatsoever from any body or from any place whatever; and that ultimately the kinetic energy so developed is—because those particles or bodies of water are not free to move but are subject to various frictions and impacts—converted into heat, a form of energy on which gravitation has no direct effect. Whence is this enormous amount of energy derived? What form of previous energy does it represent? From where and from what bodies was it transferred to the waters?

33. I am perfectly aware of the fact that truly great men, physicists, astronomers, and mathematicians, beginning with Kant, I think, and ending with Lord Kelvin have long ago sought for, and apparently succeeded in giving, a satisfactory answer to all the above questions, with respect to the source of the tidal energy.

This answer is generally expressed by these great men in something like the following:—

The tidal energy *must* be derived (mainly) from the earth's moment of momentum of rotation; the earth's angular motion of rotation, therefore, must be getting continually diminished by the tides (tidal friction), and the day must lengthen.

Professor Young, for example, in his "Elements of Astronomy," Art. 280, says:

"From what source does this energy come? The answer is, that it *must be* derived mainly from the earth's energy of rotation, and the necessary effect is to lessen the speed of rotation and to lengthen the day."

Let the reader take particular note of the expression "must be," used by Professor Young in this connection. Similar expressions, or something implying necessity I found in nearly all authorities whom I consulted on the subject of the source of energy of the tides, when giving the above answer. And this is very significant, as it shows the mode of reasoning upon which the answer is based. It is in fact this: Since the tide is subject to friction, and since friction developes heat, molecular energy, the continual motion of the tide, therefore, continually developes molecular energy. But since, by the law of conservation, no form of energy can be produced de novo, this heat *must* therefore represent some previous form of energy, and *must,* therefore, be derived from some source, and therefore, it *must be* derived either from the kinetic energy of the earth or of the moon, the two bodies concerned in the phenomena of tides.

Lord Kelvin, in expounding the theory of the retardation of the earth's rotation, repeatedly bases his conclusions principally on the law of conservation of energy.

"Even this abstract theory of energy teaches that if there is resistance of any kind (against the tidal motion of the waters for instance), that resistanve must react upon some body, and take from that body, or from bodies connected with the phenomina, energy." (Popular Lectures and Addresses, Vol. II, pp. 17.)

Again, on page 19, he says: "The theory of energy declares, in perfectly general terms, that as there is frictional resistance there must be loss of energy somewhere."

On page 20, after showing that the friction of the tides give rise to heat, he says:—

"Well, now, the end, where it (meaning the energy) leaves our earth to be dissipated through space, is heat. The beginning, to which we can at present trace the first source of that energy, is the motions of the moon and earth."

Professor George Howard Darvin, in his admirable work, "The Tides," after proving that the motion of the tide is subject to friction says:—"Now, all moving systems which are subject to friction gradually come to rest. A train will run a long way when the steam is turned off, but it stops at last, and a fly-wheel will continue to spin for only a limited time. This general law renders it certain that the friction of the tide, whether it consists in the swaying of molten lava, or of an ocean, must be retarding the rotation of the planet, or at any rate retarding the motion of the system in some way.

"It is the friction upon its bearings which brings a fly-wheel to rest; but as the earth has no bearings, it is not easy to see how the friction of the tidal wave, whether corporeal or oceanic, can tend to stop its rate of rotation. The result must clearly be brought about, in some way, by the interaction between the moon and the earth." (Chap. XVI, pp. 264-265.)

It is thus seen that the principal corner-stone in the foundation of the theory of the retardation of the earth's rotation, is nothing else but the generalized law of conservation of energy. The physicists have absolutely failed to offer any direct proof that the earth's energy of rotation is transferred to the waters of the ocean, and constitutes the tidal energy, or at least the heat developed by tidal friction; no, this they have failed to prove; they only infer that it *must* be so, because there is no other possible source from where the tidal energy might be derived, and because energy cannot be produced de novo "by any agency known to man."

34. It is a most noteworthy and rather surprising fact, that both Lord Kelvin and Professor Darvin, although starting with the assertion that wherever there is frictional resistance there must be loss of kinetic energy due directly to the friction, as seen from the instance of the fly-wheel brought forward by Professor Darvin—both of these authorities, I say, end most unconsequentially by asserting that the retardation of the earth's retation is caused, not directly by friction, but by an entirely different cause, which cause is in its turn caused by friction.

They claim that on account of friction the tidal protuberance does not exactly follow the moon in its apparent diurnal motion but gets belated, and is thus thrown eastward from the line joining the centres of the earth and moon; and that the moon, attracting this protuberance in a westward direction—and because she attracts the protuberance directed towards her more strongly than the opposite protuberance away from her—pulls the earth westward, thus diminishing the earth's rotation. And, because action and reaction are equal and opposite, the tidal protuberance pulls the moon eastward, thus accelerating the moon's velocity.

Professor Darvin says: "It follows from this that the primary effect of fluid friction is to throw the tidal protuberance forward, and the secondary effect is to retard the planets rotation." (The Tides, Ch. XVI, pp. 268.)

This, as I said, is rather surprising; for it is altogether out of the common that friction should not produce heat directly at the expense of the kinetic energy of the system, but through the intermediaary of gravitation, as is claimed in this case.

It might be asked: If by reaction the moon's velocity becomes increased, then, of course the motion of rotation lost by the earth is gained by the moon; there is no motion, then, lost to account for the tidal energy? But it should be remembered, that whenever there is a transfer of motion from one body to another, according to the law of action and reaction, there is always a loss of vis viva or kinetic energy; and it is probably to this loss that the above authors refer, when they say that the tidal energy is due to a loss of the earth's rotation. It should be remarked in passing, though, that it is more than uncommon for any loss of vis viva to occur through the action and reaction of gravitation.

35. The above is, I believe, a fair though very simple presentation of the nucleus of the theory of tidal retardation, as maintained by all great scientists of the present day; and I do not know on what ground I should ask to be excused for daring to doubt and, even more, to criticise it as far as it is understood to account for the origin of the tidal energy. Still, I will risk it.

Let me ask, then, what has the energy of rotation of the solid earth to do with the pure rise and fall of the tide, and with the motion (ebb and flow) resulting from them? True, it is, in a sense, the occasioning cause of the tidal motion; as it is through the rotation of the earth and ocean, that the alternate predominance of gravitation, mentioned above, is rendered possible; but it cannot possibly be conceived as the efficient cause of the "rise" and "fall," nor of the motion resulting from them. The earth's rotation, plainly, can not cause any rise nor fall nor any motion relative to the earth in any object on the earth. The earth's rotation plays the same role in the production of the tidal motion, as the motion of the "slide valve" on the cylinder of a locomotive plays in the production of the motion of the train. It is by the up and down motion of the slide valve that the alternate admission of steam to one or the other side of the piston is rendered possible. But it would be absurd to assert, that the train moves by, and at the expense of, the motion of the slide valve. Is it not similarly absurd to assert, that the motion of the tide is produced by and at the expense of the energy of the earth's rotation?

It is absolutely immaterial to us whether the rotation of the earth does or does not suffer any diminution or increase through the EFFECTS of tidal friction; or whether as a consequence the day does or does not shorten or lengthen. If the earth does suffer any diminution in its rotary motion through the tides, this arrested motion is probably converted into heat like any other arrested motion and like the arrested motion of the tidal wave. Moreover, the amount of retardation of the earth's rotation is thought to depend on the actual position of the tidal protuberance in relation to the moon or sun; but this position is extremely varriable; it is, indeed, so very variable that it sometimes produces acceleration of the earth's rotation instead of retardation. Further, the greatest part of the friction force of the tides can have no effect on the rotation; because while some portion of water moves westward thereby tending to retard the earth's rotation, another portion of the water at the same time moves eastward, thereby tending to increase the earth's rotation; these effects consequently cancel each other. If so, then the rotatory energy of the earth lost is represented by the difference only of the energies of those two moving portions of water and not by their sum. And how can a difference of two quantities be said to represent their sum?

From what has been said it is evident. firstly, that since the earth's rotation is not the efficient cause of the tide, it is absolutely unreasonable to assert that the energy of the tide represents any amount of rotational energy supposed to be lost by the earth; and secondly, that the quantity of rotational energy supposed to be lost by the earth bears a very small and unsteady proportion to the quantity of heat finally developed through tidal friction; thirdly, that "as the earth has no bearings, it is not easy to see how the friction of the tidal wave, whether corporeal or oceanic, can tend to stop its rate of rotation" (Darvin), we must suddenly and unexpectedly turn around and say that the friction does not directly diminish the earth's rotation, but that "the result *must* clearly (?) be brought, in some way, by the interaction between the moon and earth" (Darvin). And this, I say, is a most unreasonable and almost absurd thing to account for the origin of the energy (heat) which, we positively know, is developed directly by the friction of the tide against the earth—to account for it, I say, by the loss of motion, caused not by that friction, directly, but by some mysterious action and reaction between the "tidal protuberance" and the moon, which moon is an entirely extraneous body, and has nothing to do with the friction between the tide and the earth.

Fourthly, that accepting this action and reaction between the protuberance and moon, the greatest part of the earth's loss of rotational energy is invested, as we have pointed out above, in the increased velocity of the moon's revolution, and cannot account for the tidal energy. It is, only, the vis viva lost in the process of action and reaction, that remains to account for the tidal energy.

It is a matter of great surprise that Lord Kelvin, in his lecture "on geological time" from which we have quoted above, does not at all allude to this: that only a small part of the supposed loss of the earth's rotational energy can be employed to account for the tidal energy.

Be it as it may, I say, that this supposed loss of vis viva through action and reaction, when the agent through which that action and reaction takes place, is gravity—is a most suspicious thing. When a body, moving with greater velocity, strikes a body moving with lesser velocity, and imparts to it part of its motion, we know that there occurs a loss of vis viva, which is converted into heat. But that one body attracting another body in motion should transfer to itself part of that motion, thereby lessening the motion of that other body; and that in this transfer vis viva should be lost,—is, to say the least, a thing to be looked at with suspicion.

36. Above all, I say, that this theory of tidal retardation not only fails in its purpose of accounting for the origin of the tidal energy in conformity with

the law of conservation, but that in reality it involves a contradiction to that law.

The theory of tidal retardation claims, that because there is an elevation of the waters of the ocean of about two feet in height, constantly situated eastward of the line joining the centres of the earth and moon—the attraction of the moon on this asymmetrical elevation actually turns the earth around its axis in the direction of the attraction, that is to say, from east to west, thereby diminishing the earth's normal rotation from west to east. Further, that by reaction as much momentum as is taken off from the earth's rotation is added to the motion of the moon in her orbit. Further, that in this transfer of the motion of the earth to the moon a certain amount of vis viva is lost similar to what occurs in the collision of bodies; and it is this certain amount of vis viva lost that constitutes the energy of the tides, and which is finally converted into heat and dissipated into space.

Now, I do not see any reason why this mode of reasoning should not be applicable to any other asymmetrical elevation on the surface of the earth. Take mount Etna, for instance, and let us try to apply to it the same mode of reasoning that was applied to the tidal elevation and see to what consequences it will lead us.

When mount Etna is east of the line joining the centres of the earth and moon, the attraction of the moon on it must, as is the case with the attraction on the tidal protuberance, have the same effect of diminishing the earth's rotation and of increasing, by reaction, the velocity of the moon's orbital motion. Further, a loss of vis viva must in this case also be supposed to occur exactly as in the case of the tidal protuberance, for, for the time being, there is absolutely no difference between the two cases. Now, in the case of the tidal protuberance this vis viva was supposed to constitute the tidal energy which is eventually converted into heat. In the case of mount Etna, as there are no tides, we are constrained to assume that the vis viva thus lost is directly converted into some form of molecular energy (heat).

Now, when mount Etna passes across the line joining the centres of the earth and moon and occupies a position westward from that line, a reverse action must take place: namely, the moon then pulls mount Etna eastward, thereby accelerating the earth's rotation; thus restituting to the earth the amount of rotational motion taken off when mount Etna was eastward; similarly, by reaction, the moon loses the amount of its orbital velocity gained by reaction when mount Etna was eastward of the line. But in this transfer of motion from the moon to the earth by action and reaction there must also occur a loss of vis viva, which must also be supposed to be converted into heat.

So, then, whether mount Etna is east or west of the line, a certain amount of molecular energy (heat) must be produced by the attraction of the moon on it; and this, without any loss of motion either on the part of the earth or the moon; that is to say, a creation of heat out of nothing, contrary to the law of conservation!!

I cannot see any escape from these conclusions if the reasonings employed in the theory of tidal retardation are to be strictly adhered to.

Before I leave this article I must remark that the idea of the vis viva lost on the transfer of motion from the earth to the moon by action and reaction is entirely my own, and I failed to find any allusions to it in the authorities I consulted on the subject, except perhaps in the Encyclopedia Britannica under "tides." But I was absolutely constrained to assume it in order to give any countenance at all to the theory of tidal retardation, as far as it claims to account for the origin of the tidal energy. For, if not, then the whole kinetic energy lost by the earth is by reaction gained by the moon, and there is, consequently, no loss of kinetic energy at all, and, consequently, no energy to account for the origin of the tidal energy, which is in the end converted into heat.

37. On the whole, I think that the theory of tidal retardation, as far as it claims to account for the origin of tidal energy, if examined with a clear unprejudiced mind, will inevitably be found to be composed of a web of impossible conceptions and contradictions; and has absolutely nothing in its merit, except the great and well deserved renown of the great men, who are connected with it; and that it utterly fails to account for the origin of the tidal energy.

38. Where does the energy of the tide then come from? Is it not as clear as daylight that this energy is born, so to say, de novo through the action of gravitation? Or do you rather prefer to delegate this question to the province of the mysteries of nature, and say it *must* come from some source of actual energy although we are ignorant of what source? Do you rather prefer to plead total ignorance than give up your notions of the indestructibility and increatibility of energy? Can you shut your ears against the holy voice of Nature, the holy voice of the Almighty uttered through nature, thundering at you through the

mighty billows of the ocean, that your notions are absolutely and utterly false? Can you shut your mental eye against the effulgent ray of light and truth that comes from the sun and moon? Why? why should we do so? Because we, educated and highly cultivated men of the twentieth century have persuaded ourselves that so and so it *must* be? Ask the ages that have gone by, ask even Newton, and they will tell you differently. Ask the peasant, who is nearer to God and to nature than we are, and who better understands nature's simple language, and he will tell you that your notions *must* be false. Inquire of heaven and earth and they will most loudly protest against your tyranny. Tides and conservation of energy! Are they not contradictory? One of them is assuredly false. Will anyone dare go against the tides in order to save an arbitrarily assumed principle? Assuredly the tide is a living testimony that the law of conservation as understood at present is untrue!

39. I might have been a little over enthusiastic and over self-assertive in what was said with respect to the theory of the tidal retardation and the origin of the tidal energy. But as it springs from a deep conviction of the truth of those assertions I hope to obtain the reader's forgiveness.

One word more before we quit this subject. Should the reader please to disagree with me on the subject of the tides, and, rather, prefer to side with the theory of tidal retardation which has so much weight of authority on its side—then, let me remind him of what was said before, that the assertion made in Art. 29 of this section is based, not solely nor principally on the tidal phenomena, but on some grander inductions and deductions, the truth of which cannot at all be doubted, as will be shown in Book IV of the 1st part of this volume and in the second part thereof.

40. Before concluding this section and as an introduction to the next section it will be fit to say a few words regarding the old name for kinetic energy: "vis viva."

The term "vis viva," otherwise "vis motrix," was first employed by Leibnitz in 1695 to signify the force of a body in actual motion in contradistinction to "vis mortua," which means statical force: pressure or tension.

Our modern term "energy" is evidently a literal translation of "vis viva," since energy literally means "living force," as we had occasion to remark above.

With regard to the measure of this "vis viva," Leibnitz was also the first to assert that it is proportional to the second power of the velocity, in opposition to Des Cartes, who maintained that it is proportional to the first power of the velocity. A hot controversy ensued between these two great men and between their disciples, which lasted for a period of about thirty years.

The following is a quotation from an old volume published in 1855 of "A Collection of Problems in Theoretical Mechanics," by Wm. Walton, A.M.: "The memorable controversy of the vis viva, after raging for a space of about thirty years, was finally set to rest by the luminous observations of D'Alembert, in the preface to his Dynamique, who declared the whole dispute to be a mere question of terms and *as having no possible connection with the fundamental principles of mechanics.* Since the publication of D'Alembert's work, the term vis viva has been used merely to signify the algebraical product of the mass of a moving body and the square of its velocity while the words moving force (vis motrix) have been universally employed, agreeably to the definition given by Newton in the Principia, in the signification of the product of the mass of a body and the accelerating force to which it is conceived to be subject, no physical theory whatever in regard to the nature of force being supposed to be involved in these definitions."

It is important to note that originally both terms vis viva or energy and vis motrix or moving force or simply *force* were indifferently applied by both Cartesians and Leibnitzians to designate the principle of activity of a body in motion—the dispute only being about the measure of that principle as expressed in units of velocity. It was only in the course of time and after the controversy was finally set to rest by D'Alembert that the meaning of the two terms became distinct and non-interchangeable; the term vis viva carrying with it by association the idea of its measure as maintained by Leibnitz, the originator of the term; and vis motrix connoting the measure of the principle of activity as maintained by the Cartesians and afterwards by Newton.

BOOK II.

FORCE
AND
ENERGY.

THEIR RELATIONS AND MEASURE.

BOOK II. FORCE AND ENERGY, THEIR RELATIONS AND MEASURES.

SECTION I. FORCE AND ITS MEASURE AS RELATED TO ENERGY.

1. In all modern works on physics, it is customary first to treat of "force" and then, after a more or less exhaustive consideration of force, to add a special section on energy; thereby virtually affirming the distinct nature of the two things or terms: force and energy.

Now, the question arises: What is force as compared to energy, and what is energy as compared to force? Do they refer to two separate things, or do they refer to one and the same thing under different aspects?

No one, I think, will allow that some phenomena of nature should be considered as manifestations of energy, and not of force, and others as manifestations of force and not of energy. Clearly, then, force and energy must refer to the same thing (under different aspects). The definition of force, also, is identical with the definition of energy in its wider sense. (See Art. 3, Sect. III, Book I.) Moreover, the terms force and energy were originally applied by both Cartesians and Leibnitzians to the same thing; no one ever dreaming of any distinction between them. (See Art. 40, Sect. II, Book I.)

2. When we speak, then, of force and energy, we refer to one and the same "principal of activity" or "capacity for effecting change" or "cause of change"

—choose whatever phraseology you like best, since of the essential nature whether of force or of energy we know nothing. (See Arts. 1, 2, Sect. I, Book I.)

Nay, more, if you are a follower of Hume, and deny "causation" altogether, I will agree with you, that force as a cause of change is only a "convenient form of speech" and has no real existence; but I claim the same for energy, since energy as "capacity for doing work," that is, of producing change, involves the same idea of "causation."

I would say, however, that in physics, the science of causes and effects, the doctrine of "invariable sequences" is almost suicidal. What sense will your measuse of force and energy have according to this doctrine? The measure of what? Of invariable sequence? Is not this ridiculous?

In physics, at least, we ought to forget about all metaphysical speculation, and deal of causes as of "something," and, moreover, as of "something" measurable; neither ought we to be over sensitive to the use of such expressions as "exertion of force," "action of force," etc. And it seems to me very inconsistent of some physicists to use such expressions marked with signs of quotation, to indicate their irresponsibility for the coinage of such expressions.

Neither ought we to dread to approach the sanctum sanctorum of physicists "the physical nature of force." Since the days of Newton, who expressed his unwillingness to speculate about the nature of gravitation, physicists have generally exhibited an extreme forcephobia, so to speak. So extreme is this forcephobia with physicists that all quantitative expressions referring to force or energy are looked upon merely as algebraical or symbollical, that is, as dead letters, as far as the physical world is concerned; their only use being for the purpose of certain mathematical speculations. (See quotations in Art. 40, Sect. II, Book I.) And this leads to great mental confusion. True, the nature of force will, of necessity, always remain inscrutible, no matter how strong our desire and efforts might be to scrutinize it. Even Le Sage's theory of gravitation does not claim to explain the nature of gravitation as a force. It only renders gravitation an effect of another force, (the velocity of particles), which force, in its turn, is inscrutable. But by rendering gravitation an effect, it renders it to some extent comprehensible: for, while causes are entirely incomprehensible, as far as their essential nature is concerned—effects may be considered as to some extent comprehensible. The saying of God to Moses: "You shall see my back, but my face cannot be rendered visible" may be translated thus: You shall partly comprehend the effects or sequences of causes, including the cause of all causes, but the antecedents of those effects, that is, the causes of those effects cannot be rendered comprehensible as causes. For while causes, as causes, are essentially active, our minds are essentially passive; and it is absolutely impossible for the passive to comprehend the essentially active.

But while this is so, we ought not on this very account, and in a very paradoxical way, negate "force" altogether, and deal with its attributes as the attributes of a non-existence; on the contrary, we ought to conceive those attributes as attributes of a great inconceivable "something."

3. To return to our subject. Since force and energy refer to the same thing, what, then, is their difference? Under what aspect do you call the thing (or condition of things) force, and under what other as-

pect do you call it energy? Especially is this question striking, when applied to the measure of the two, force and energy. Under what aspect do you measure the *thing* by mass and velocity, and under what other aspect do you measure it by mass and the square of velocity? It does seem that physicists have consciously, or unconsciously, adopted the two contradictory measures of force, as maintained on one hand by the Cartesians, and on the other by the Leibnitzians. What has compelled them to do so?

4. Two answers to this question are possible:—(a) That the measure of energy is actually the measure of the "something" absolutely considered; that is to say, it is the true measure of the *something* itself, as it is in itself, out of all relation to anything else; while the measure of force is the measure of the *something*, relatively; that is to say, it is the measure of the *something* as contemplated relatively to something else. (b) The converse of the above, namely, that the true absolute measure of the *something* is by mass and velocity, while the measure of energy is relative.

Under both suppositions the nature of this relativity would have to be stated.

5. Physicists are generally loath to discuss this question, or to state the relation of force and its measure to energy and its measure in definite and intelligible terms, in order to make us clearly understand why two measures are necessary or possible for one and the same thing, or, if of different things, what constitutes that difference? If they do happen to touch on this question, they touch on it but lightly, and answer it, as though in passing, by a few hardly intelligible words, just enough to pacify the over-curious.

6. Now, experiments show us, that the effects of a moving body, when those effects are of the molecular type: heat, light, electricity, etc.—are, invariably, proportional to the square of the velocity. Only when the moving body produces molar effects, that is when it communicates its motion to another body, or bodies, in motion or at rest—it is only then, that we notice that its effects, as far as molar motion is concerned, are regulated by the measure of force; that is to say, by mass and velocity. Force and its measure, then, has its applicability only to the interaction of bodies in motion, as far as molar motion is concerned; or, more generally, the measure of force regulates the laws of molar motion, only, viz: the law of action and reaction, and the law of composition of forces (which is regulated by velocity and not by its square) with their various corollaries. But even in the domain of molar motion there are in most cases, at least, molecular effects (heat) accompanying the molar effects produced by the interaction of bodies in virtue of their actual motion; as is the case, for instance, in the collision of bodies; and the sum total of the molar and molecular effects taken together is invariably found to correspond to the measure of energy.

Now, because of this, most physicists adhere to our first alternative of the previous article, namely, that the measure of energy is the true absolute measure of that *something, principle of activity,* or whatever you may choose to call it. And they affirm this when they say "there is nothing in the universe but matter and energy."

But what of force and its measure? Why should the molar effects of bodies, as we have just explained, be regulated by a measure not corresponding in any way to the measure of the *something,* which is the cause of those effects? Of this they dispose very lightly. Thus Tait says, in this connection: "Force may be regarded as the measure of the *tendency* of energy to transform itself." I confess, that this phrase fails to convey to my mind any definition of force as contradistinguished from energy. The term *tendency,* in itself, conveys a very hazy conception to the mind; and a measure of *tendency* is a conception which my mind, at least, can hardly grasp. Let the candid reader judge for himself, whether this phrase contains the slightest allusion to a solution of what it is intended to solve.

That force and its measure, according to which the law of action and reaction is regulated, by which the law of composition of forces is governed, and to which the motions of the heavenly bodies in their orbits are constantly subject, while describing equal areas in equal times, etc.—that this force and its measure should be defined as a measure of any tendency, especially as a measure of the tendency of energy, is most incomprehensible.

But even accepting this definition of force as related to energy, does it solve the difficulty? Positively not. For, a *tendency* of energy, or, for that matter, any property or quality or energy, must be proportional to the amount of energy; and, since energy is proportional to the square of velocity, the *tendency* of energy or force must, similarly, be proportional to the square of velocity. Why should force, an effect or property of energy, have another measure, when measured by velocity, than energy its cause? Is it not a contradiction to the apriori undeniable truth, that effects or properties must be proportional to their causes?

Tait further says, "Force is a mere phantom suggestion of our muscular sense." But it may be asked, why then do physicists give this phantom a measure? Further, if force is a mere phantom, then is the whole universe—the motions of which are effected according to the laws of force and not of energy, as we have pointed out—a mere phantom, a conclusion hardly acceptable to the physicist. Moreover, if because it is suggested to us psychologically by our muscular sense, you, therefore, call force a mere phantom,—energy likewise must be termed a mere phantom; because it, or its idea, is similarly derived from our muscular sense.

Daniell, apparently trying to solve the same difficulty, says:—

"By a convenient form of speech a given force is said to act on a given body, and to impart to it a given velocity. But it must be constantly borne in mind that force is not a physicial entity, and can never be measured until we already know absolutely, or by comparison, the mass acted upon and the acceleration imparted to it."

(See Gage's Principles of Physics, Art. 73.)

He apparently belongs to the school of Hume. But all we need say is, that the same phraseology may, and ought, according to that opinion, be applied to energy as well as to force.

7. The second alternative of Art. 4, namely, that the true absolute measure of the *something* is by mass and velocity, while the measure of energy is relative—is hardly to be thought of, since there is no absolute conservation of force, but there is, in a limited sense at least, an absolute conservation of energy. And right here let me answer to a statement that might be made, and may throw us into confusion. It may be said: Why, the measure of force by momentum takes ref-

erence of direction, and may, therefore, be either positive or negative; while the measure of energy is always positive, and has no reference to direction. So, indeed it is; but what of that? I ask, why should it be so? Why, when you take direction into consideration, should the measure of the same active principle be different from the measure of the same active principle when you don't take such direction into consideration? Besides, the possibility of the positive and negative sign being applied to the measure of force is only a consequence of the law of action and reaction, which is regulated by the measure of force. But the measure of force, itself, is a signless quantity, and is always positive. We call the momentum of one body positive or negative, as compared with the momentum of another body moving in an opposite direction to the first body, according to the law of action and reaction. But the momentum of each body separately is, indeed, a signless quantity. Positive and negative momenta are only employed mathematically for determining the condition of the centre of inertia and of the law of conservation of momentum, as deduced from the law of action and reaction. All quantities of nature are essentially signless; and it is only in our forming conceptions of them, as compared with one another, that we call one positive and another negative, for the convenience of calculating the results of contrary effects.

By the way, the conservation of momentum is no conservation at all in a physical sense; it is only a summing up of contrary effects, as far as molar motion is concerned, and is a direct consequence of the law of action and reaction.

8. From what has been said above in Arts 6 and 7, it is rendered apparent that the actual true and absolute measure of the *something* is undoubtedly that of *energy,* that is to say, by mass and half square of velocity; while force and its measure, though having reference to the same something, are related to molar effects produced by the actual molar motion of bodies.

I say, "produced by the actual molar motion" advisedly; for the measure of molar effects produced by other than kinetic energy, that is by molecular energy, is not that of force, but of energy. If a certain amount of molecular energy, say, for instance, the energy of chemical affinity in gun powder—produces in a body, a rifle ball for example, of mass (M), a velocity (V), then, 4 times the quantity of powder will produce in the same ball not 4 times the velocity (V), as it ought to be according to the measure of force, but it will produce $V \sqrt{4} = 2V$, only twice the velocity (V).

The following is an extract from Hutton's "Mathematics," very well illustrating my meaning:

"Practical rules in projectiles"—(gun projectiles).

(a) "The velocity varies nearly as the square root of the charge (quantity of gun powder) when the shot are the same. That is, if (V) and (v) are the velocities and (C) and (c) the charges; then,

$$\frac{V}{v} = \frac{\sqrt{C}}{\sqrt{c}} \text{ nearly}$$

(b) With equal charges the velocity varies inversely as the square root of the weight. That is, if

(B) and (b) are the weights of the two shots, then

$$\frac{V}{v} = \frac{\sqrt{b}}{\sqrt{B}} \text{ nearly}$$

(c) When unequal shot are projected with unequal charges, then,

$$\frac{V}{v} = \frac{\sqrt{C}}{\sqrt{c}} \times \frac{\sqrt{b}}{\sqrt{B}} \text{ nearly, etc.}$$

It is thus seen that the molar effects of a molecular force or energy are regulated according to the laws of energy, not of force.

9. Now, there could not be a better example of an impulsive force than that of a discharge of gun powder; and if so, the conclusion reached just now actually amounts to this: That the measure of an impulsive force (other than actual molar motion), with respect to a molar effect, is by mass and equare of velocity—a conclusion entirely at variance with the statements of all books on physics and mechanics which make the measure of an impulsive force to be that of momentum. (See Art 15, Sect. I, Book I.) It seems impossible otherwise than to consider these statements of books on physics and mechanics, respecting the measure of an impulsive force, to be an error that has unobservedly slipped in into scientific treatises. Because the measure of a constant force, or, as I prefer to call it, an inextaustible force, like gravity, is undoubtedly by the velocity it produces in a unit of time, or, taking mass into consideration, by the momentum it generates in a unit of time. (See Art 16, Sect. I, Book I); that is to say, that a force four times *as*

strong as another force will produce four times as much velocity in a unit of time as that other force; or, if you so please, will generate four times as much momentum in a unit of time as the other force—the inference is at once drawn that the same must hold good in the case of an impulsive exhaustible force also; that is to say, that a force four times *as large* as another force must produce four times as much velocity as the other force (in one and the same mass); or must generate four times as much momentum as the other force.

But the inference is not correct. For it must not be forgotten that the measure of a constant or unexhaustible force is a measure of quality only: of intensity, but not of quantity; while what we mean by measure of an impulsive force is quantitative. (Read carefully Arts 12-16 of Sect. I, Book I.)

At first sight it may appear that the inference from a qualitative measure to a quantitative measure, in this instance, is entirely legitimate; but we hope to point out, as we proceed, the reason why the inference is not legitimate. At present, we are satisfied with appealing to the fact, and the fact is, that four times an impulsive force, provided it is not the actual molar motion of a body, produces not four times the velocity in one and the same body, but $\sqrt{4}$ times the velocity, as seen in the first equation on projectiles quoted above.

Even the two greatest physicists of England, Lord Kelvin and Tait, have fallen into the same error; for, in their admirable treatise on "Natural Philosophy," while discussing the second law of Newton, they say:

"If equal forces be exerted on different bodies, the change of velocity produced *in equal times* must be inversly proportional as the masses of the various bodies."

(*Natural Philosophy,* Art. 258.)

They then illustrate this and say, "this is approximately the case, for instance, with trains of various lengths started by the same locomotive" (the same steam power?). But this is almost an unpardonable error; for the same amount of steam energy cannot generate, in a train, say, four times as long, one-fourth of velocity only, but it will actually produce one-half of velocity, as is evident from the second equation on projectiles quoted above.

This subject of the measure of an impulsive force shows us the great chaotic confusion prevailing in the state of our comprehension of the laws of force and energy. Men of science rest satisfied with the assertion that the measure of energy is such and such, and that the measure of force is such and such; but they do not consider it worth while to define their position clearly, and ascertain under what condition does nature act according to the laws of force, and under what other conditions it acts according to the measure of energy. Even the difference as to molar and molecular effect, mention in Art 6 of this section—even this I fail to find stated definitely and expressly by any of the great authorities which I have access to, such as Ames, Stallo, Tyndal, Gannot, Kelvin, Tait, and Stewart.

I am not deficient in the profoundest respect for those great and powerful minds—far be it from me. I only state what seems to me to be true; and, as I said above, I honor the truth above everything else.

10. Let us sum up the argument and conclusion arrived at in this section.

On close examination we find:

(a) That the molecular effects of a molar force and the molar effects of a molecular force are invariably regulated by the measure of energy.

(b) That the measure of force is applicable only within the domain of molar motion exclusively, and it only regulates the law of action and reaction, principally, and the law of composition of forces, and their several corollaries, as far as molar motion is concerned. I must add here, that the measure of force regulates the law of action and reaction as far as molar motion is concerned, no matter what the cause of that action may be; whether it be molar motion or another cause, mutualy acting between two bodies and tending to cause molar motion only in both of them, as is the case with gravitation or other attractions and repulsions.

(c) That even within the domain of the measure of force, as specified, there are molecular effects accompanying the molar effects, if those effects are produced by actual molar motion; and that the sum of the molar and molecular effects is invariably equal to the *energy* of the molar motion producing these effects. And thereby it is proved that the actual absolute measure of the "something" is by mass and square of velocity, and that the measure of force is, as we termed it, relative.

(d) The only question remaining is—why should it be so? Why should energy act, within the domain of molar motion, not acording to its own measure, but acording to a totally different measure? At present we leave this unanswered. In a subsequent section we will attempt to answer this question together with another problem about energy, to the discussion of which we will devote the next section.

SECTION II. ENERGY AND ITS MEASURE.

1. The measure of energy is, as we have seen above, the true and absolute measure of that *something* to which all change is due; and the measure of energy is by mass and half square of velocity: $M.\frac{V^2}{2}$. Innumerable experiments have confirmed this, and especially the experiments in thermo-dynamics by Joule and others. This is the fact. What we intend to do in this section is to examine the logical explanation of this fact, why this should be so.

2. Physicists, in order to explain this, have defined energy as the "capacity to do work," thus introducing an indefinite new term, *work;* and then they define work as involving two ideas, that of force and that of space; and finally they say that the measure of work, which is at the same time the measure of energy, must be the product of the two notions involved in the idea of work, as they arbitrarily define it; that is to say, it must be measured by the product of force and space (FS); and this is equivalent to $\frac{V^2}{2}$. (See Arts 7 and 8, Sect. II, Book I.)

Physicists, in fact, adopt the reasoning of Leibnitz, who, I may say, divined the actual measure of vis viva prior to all experimental proof. But because this is so, it does not at all necessarily follow that Leibnitz's reasoning of arriving at the measure of vis viva (namely: of multiplying the intensity of the force by the space through which it works), must necessarily be adopted as an explanation of the fact taught *us* by experiments. Because the result of a certain mode of reasoning seems to agree with truth, it does not follow that that mode of reasoning must necessarily be unfaulty.

In the following, we hope to prove:

(a) That the mode of reasoning adopted by physicists as an explanation of the measure of energy is faulty.

(b) That even the results arrived at by such reasoning do not, in certain cases, agree with fact.

3. In the first place, let us clearly grasp the meaning of the physicists. They say that work or energy is, and ought to be, measured, firstly by the intensity of the force (F) doing the work or exerting the energy; and, secondly, by the space (S) through which the force (F) works; as though the force (F) were in itself the immediate cause of the displacement of the body through the space (S) *during the time of action*. Now, this is far from being correct. For the displacement of the body during the time of action is not due directly and immediately to the *force* in itself; but is due to the inertia continually acquired by the body, from the beginning of the time of action till its end. But the continual acquisition of inertia takes place continually after the cessation of the past action of the force, through which the inertia is continually acquired.

Newton says: "An impressed force is an action exerted upon a body, in order to *change* its state either of rest or of moving uniformly forward in a right line.

"This force *consists in the action only,* and remains no longer in the body, when the *action is over.*

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For a body maintains every new state it acquires by its vis insita only." (*Principia,* Def. IV, Book I.)

Lord Kelvin, also, says thus:

"Force is wholly expended in the action it produces, and the body, after the force ceases to act, retains by its inertia the direction and the velocity which were given to it."

(*Natural Philosophy,* Art 217.)

Newton, as well as Kelvin, evidently means by the term *action,* in this connection, the *change* of state of inertia of the body acted upon.

It is therefore evident that force does not directly cause any translation through space; it only changes the state of inertia of the body. And if a force acts during some interval of time, it successively changes during that interval the state of inertia of the body acted upon, but does not cause the displacement of the body through the space (S) passed by it during that interval of time. This, the displacement, is caused by the inertia of the body. Force, therefore, cannot be said to *work through any space;* and the expression "the product of the force (F) by the space through which it works" is, therefore, incorrect. For force has no direct relation to space at all; whether to that passed by the body during *the time of action,* or to that passed by it afterward.

Furthermore, if you conceive the force (F) to act through the space (S), passed by the body during the time of action, then, since the body passes that space (S) in a *continuous* line, you are forced to conceive the force (F) to effectively act continuously likewise; but we have proved above (Art. 6, Sect. I, Book I), that a continuous effective action is only an ambiguity of language and is inconceivable. Force cannot act effectively continuously. Inertia must. And, if force cannot be said to work through space, then, the measure of energy by force and space has no sense.

4. In the second place, if we carefully analyze the formula FS, we cannot fail to discover that it is composed of two heterogeneous units: F means so and so much *velocity* in a *unit of time,* while S means a number of units of *space.* If F should be the measure of the intensity of the force in a unit of space and S should be understood as expressing the number of such *units of space*—under such supposition would the product FS be entirely legitimate and logical. But to multiply F, which is the measure of the intensity of the force in a *unit of time,* by the number of an entirely different unit, S, which is the number of *units of space,* and to call this product the measure of anything at all, seems to me to be highly unmathematical.

As an illustration, let us suppose that we hired a man to lift a certain weight to an indefinite height, and suppose that we were to pay the man for his labor at the rate of thirty cents per second. Now, what is the measure of the laborer's wages? Clearly it is 30 × the number of seconds during which he exerted himself, and not 30 × the number of feet through which he has exerted himself, for, in that case, the number 30 would necessarily mean 30 cents per foot, and not per second, contrary to our supposed agreement with the laborer.

Remember that F expresses *velocity* (not space) in a unit of time (a second), not in a unit of space, and the simile becomes perfect.

5. In the third place, velocity must be considered either as a simple effect of force, or as a non-simple effect of force (compounded effect). If velocity be a simple effect of force (as physicists generally hold), then, by the axiomatic truth that simple effects are proportional to their causes, velocity ought to be proportional to the amount of force (or energy) exerted in producing it. But such is not the case, for, in reality, velocity is proportional to the square root of the amount of force exerted for producing it.

If, on the other hand, velocity be a non-simple or compounded effect of the amount of force, then, it becomes incomprehensible how it is that the intensity of force is measured by the velocity it produces in a unit of time. Since, the force and velocity bear no simple relation to each other, how, then, can one be expressed by a simple relation to the other? (See Art 13, Sect. I, Book I.)

The explanation of the measure of energy, as given by physicists, does not solve this difficulty; this difficulty, therefore, is an argument against that explanation.

6. Up to this point we attempted to prove that the *mode of reasoning* of physicists in this connection does not stand a critical examination. We will now attempt to show, that even the results arrived at by such reasoning do not agree with, or fail to explain, some facts.

Suppose a body of unit of mass, to which, somehow, a velocity V has been communicated, to be acted upon by a force F for an interval of time (t), in the direction of the previous velocity V. The velocity at the end of the time (t) will be $V + Ft$. The space passed over by the body during (t) will be equal to $Vt + F\frac{t^2}{2}$. Now, the portion of space Vt, passed by the body during the time (t), is positively due not to the exertion of the force F during the time (t), but is due to the velocity V, which the body possessed previous to any exertion of the force F on it. Therefore, if energy ought to be measured by the space passed over by the body during the time of action, multiplied by the force, then, the increase of energy in the body over and above the energy $(\frac{V^2}{2})$ possessed by the body prior to the action of the force on it, ought to be $(\frac{Ft^2}{2})$ F, for only the portion $\frac{Ft^2}{2}$ represents the space, which is due directly to the exertion of the force, but not the space Vt. Therefore, the total energy of the body ought to be $\frac{V^2}{2} + (\frac{Ft^2}{2})F$; $\frac{V^2}{2}$ representing the energy the body possessed previous to the exertion of the force, and $(\frac{Ft^2}{2})$ F, representing the energy acquired by the body through the exertion of the force F during the time (t). So it ought to be, if the measure of energy be by force and the space passed over by the body through the exertion of the force during the time of action. But, in reality, it is not so; for the total energy of the body at the end of the time (t) is, in reality, equal to

$$\frac{(V+Ft)^2}{2} = \frac{V^2}{2} + V(Ft) + \frac{(Ft)^2}{2}; \ (V + Ft)$$

being the final velocity of the body at the end of (t).

SECTION III. FORCE AND ENERGY. THEIR RELATIONS AND MEASURES DEFINED.

In this section we intend to attempt a rectification of the difficulties enumerated in Sect. I and II of this book. And it will be of great advantage to us to arrange what we have to say in this section in the form of definitions, propositions, corollaries, and scholii; for only in this way can we guard ourselves from confusion.

Definition. A number of bodies, or particles of matter, moving with the same velocity and in the same direction, constitute what is called a "system" of bodies, as far as any interaction between the bodies themselves is concerned; and any motion of the bodies with respect to one another, caused by the interaction of the bodies amongst themselves, is effected in accordance to the laws of motion, as though the bodies were not moving at all, but at rest. Such a system of bodies are therefor considered as at rest, as far as any interaction between themselves is concerned.

It follows from this, that "rest" in such cases is only relative; relative rest, and not absolute. It is the "rest" of motion: of equal and parallel motion.

In this section we will understand by "rest" any "rest" at all, whether absolute "the rest of rest," or relative "the rest of motion."

As motions differ in velocity, therefore, "the rest of motion" of one system differs, in certain respects, from "the rest of motion" of another system, whose parallel motion is of a different velocity than that of the other system. This subject will be discussed in Book IV. At present we are concerned with the laws of motion, of force and energy, of *one* system, and it is immaterial to us whether the system is at *absolute* rest or in motion with any velocity whatever.

PROPOSITION 1. Force and resistance are two correlative terms, one implying the other: without implying the exercise of force resistance has no meaning; for resistance is to *force* only, and to nothing else. Similarly, without the existence of resistance an exertion of force is inconceivable, for, if you imagine the exertion of a force on something that offers no resistance to result into an effect—that effect ought to be infinite, no matter how small the force; because the numerical relation of something to nothing is infinity. But, I say more, that the exertion of a force is, so to speak, called into activity only through something that offers resistance to its activity. And when there is no such resistance the force is not called into activity at all. It does not act.

Corollary 1. In any and all effects of force, the proportion of the quantity of force exerted to the quantity of resistance overcome is a constant quantity. I mean, that the greater the quantity of resistance overcome, the greater the quantity of the force exertd, and vice versa; or, as you might say, that the quantity of the amount of force exerted and the quantity of resistance overcome are equal.

Corollary 2. The magnitude of the effect of a force of any magnitude, or intensity, on a body, is not limited only by the magnitude or intensity of the force alone, but it is also limited by the amount of resistance which the body is capable of offering; and if the amount of resistance which the body is capable of offering be less than the capacity of the force to act—the magnitude of the effect will be limited to the amount of resistance the body is capable of offering. For, on no resistance force cannot act.

PROPOSITION 2. The exertion of a constant inexhaustible force, like gravitation, if it act effectively for any sensible time, must be continuous; but the manifestation of the effects must be conceived as taking place interruptedly; an infinitesimal interval of time separating one effect from the other. Take, for instance, the case of a body falling freely through the exertion of gravity for one second. I say, that the exertion of gravity on it is continuous throughout the second, but that the increments of velocity taking place in the body throughout the second from zero up to 32 feet—this accretion of velocity is not continuous, but takes place interruptedly, a very minute fraction of a second, say for the sake of illustration, one millionth part of a second, separating one minute accretion of velocity from the other. So that immediately after the body is set free 0.000001 of a second elapses during which the body does not move at all; then it starts to move with a velocity of 0.000032 feet, and continues so to move uniformly through 0.000001 of a second, when another increment of velocity takes place, with which velocity it continues to move through the third 0.000001 of a second, when another increment takes place; and so on.

Demonstration. In Art. 7, Sect. I, Book I we proved that a continuous effective action is inconceivable. There are, then, only two possible assumptions for explaining the mode of action of a constant inexhaustible force like gravity. Either that the exertion of the force takes place interruptedly, the effects taking place simultaneously with each separate exertion —instantaneously; or, that the action of the force is continuous, without interruption, but that the manifestation of the effects takes place interruptedly.

But the first supposition must at once be dismissed; for it is contradictory to ascribe cessation of action to any force, and especially to an inexhaustible force: if you imagine a force to cease to act, you are compelled to assign a cause for the resumption of its action, which is absurd.

On the other hand, effects cannot be conceived as continuous; for effects are changes of states of a body, and in order for a body to change its state, it must first *be* in one state before a change to another state can be conceived at all. But *to be* in a state implies duration; a body, then, must be for *some duration* in one state before it changes to another state; which proves our proposition.

Remark. In order to facilitate discussion hereafter, we will introduce the following nomenclature with reference to some conceptions arising from this proposition:

(1) *Microf*—(micro-effect)—to signify each minute increment of velocity referred to above.

(2) *Microt*—(micro-time)—to signify the minute interval of time separating one effect from the other. It is the minute duration through which a body, while under the influence of a force, abides in one state before it assumes another state. It is also the minute duration through which a force must be conceived to act before it produces a *microf;* for force must be conceived as acting in time.

(3) *Micros*—(micro-space)—to signify the minute space passed by a body under the influence of a moving force during a *microt.*

(4) *Microd*—(micro-dynamo)—signifying the minute amount of energy expended for each minute

effect, or *microf*. And since the energy expendd equals the resistance overcome (Prop. 1), the *microd* is also the measure of the resistance overcome, as will be explained in the sequel.

The microd is also the measure of the amount of energy conferred (or destroyed) by the force upon the body acted upon, while producing a microf; for the amount of energy conferred must *equal* the amount of energy exerted. These three must be equal; namely:

(1) The amount of energy exerted;

(2) The amount of energy conferred (or destroyed);

(3) The amount of resistance overcome.

Remark 2.—For purposes of calculation we will adopt the following notation:—

(a) M'f—microf.
(b) M't—microt.
(c) M's—micros.
(d) M'd—microd.

It will be noticed that M'f, M't, M's (and M'd) correspond to dv, dt, ds, of the differential and integral calculus, with this difference that while in the calculus we understand by *dv,* etc. undetermined quantities—here by M'f, etc. we are to understand fixed and determined natural quantities, as will be gathered from what follows.

PROPOSITION 3. By inertia we must understand a positive resisting force inherent in bodies by which they endeavor to maintain their present state whether of rest or of any degree of motion in a straight line. Newton gives the best definition of inertia that I am acquainted with:

"The vis insita, or innate force of matter is a power of resisting, by which every body as much as in it lies, endeavors to persevere in its present state whether it be of rest or of moving uniformity in a right line."

(Principia. Definition 3.)

Lord Kelvin and Tait, following Newton, express themselves thus:—

"Matter has an innate power of resisting external influences, so that every body, as far as it can, remains at rest or moves uniformly in a straight line. This, the inertia of matter, is proportional to the quantity of matter in the body."

But these definitions, certainly very good in themselves, lack one thing: and that is, proof that it is so. Both Newton and his followers fail to prove that this is so. And because of this lack of proof, there are many modern physicists who look upon what is called inertia as on a mere negation of power: an inability on the part of matter to change its state. So, for instance, I read in *Gage's Principles of Physics*: "All matter is inert; i. e., that bodies of matter are utterely incapable of putting themselves in motion or to stop themselves; the inability is called *inertia*." This view of the matter has its attraction in the fact that it dispenses with a *force*: force being so much abhored by modern physicists.

That inertia is an inherent resisting force and not a mere *inability*, is sufficiently proved by our first proposition: if inertia be no resistance, then, the smallest possible force exerted for any short time ought to produce an infinite velocity in the largest possible mass of matter, which is absurd.

PROPOSITION 4. The first microf produced by a force of *any intensity* on a body in a state of rest, that is, the first microf from rest and so, also, the first microd—are invariably fixed and determined quantities, no matter what the intensity of the force producing them.

Demonstration. Since the 1st microf is the first change of state of a body from a state of rest, and since the 1st microd is the amount of energy spent in producing the microf, and since the amount of energy spent is always equal to the amount of resistance overcome (Corollary 1, Proposition 1); and since the resistance in this case is due to the inertia of matter in a state or rest; and, since the inertia of matter in a state of rest is always the same—it follows that the 1st microd overcoming this invariable, fixed, and determined quantity of resistance due to the inertia of matter in a state of rest must be, like the resistance it overcomes, an invariable, fixed and determined quantity, no matter what the intensity of the frce; for a larger force cannot produce an effect larger than the inertia of rest which it is to evercome (Corollary 2, Proposition 1), neither can a smaller force produce an effect smaller than the resistance of inertia of rest; for in that case no change of state would ensue altogether; which proves our proposition as far as the microd is concerned. Now, since the 1st microd is a constant quantity, the 1st microf must also be a constant quantity.

Corollary 1. The 1st microf, the first minute velocity imparted to a body from rest by any force, is the least possible and consequently indivisible velocity; for a force so small that it cannot overcome the resistance of inertia of matter at rest will produce no change of state, that is, no velocity at all. The 1st microfs are to velocity what atoms are to matter: they are the natural units of velocity, just as atoms are the natural units of matter; and all velocity must be conceived as composed of microfs, just as all matter must be conceived as composed of atoms. There is a finite unit of velocity, just as there is a finite unit of matter. And the dictum of physicists that a *finite* force, no matter how small, must always produce an effect, "no matter how small," is entirely erroneous.

Corollary 2. Since the above reasoning is applicable also to the 2nd microd and microf, and also to the 3d, 4th, etc., that is, to any microf and microd, for the resistance of inertia of any state of matter to which any microf and microd belong is always fixed and determined—it follows, that each and every microd or microf, whether of the 1st, 2d, 3d, etc., states, is a fixed and determined quantity for itself; that is to say, that a 2-d microd and microf, for instance, cannot be sometimes larger and sometimes smaller; and so a 3d and 4th, etc.

But it does not follow from this that all microfs and microds are equal among themselves; that is to say, that the 1st microd and microf are equal to the 2nd microd and microf and to the 3d and so on; for it has not been proved that the resistance of inertia of the second state of matter is equal to the resistance of inertia of rest, nor that of 3d state, 4th, etc., is equal to that of the 2nd or to that of any other.

We will, indeed, prove in the sequel that all microfs are equal among themselves, but also that all microds differ from each other: those that belong to a state further removed from rest being larger than each and every microd belonging to states nearer to rest.

PROPOSITION 5. All microfs, no matter what the intensity of the force, are equal in magnitude among themselves, no matter how far or near the state of rest.

PROPOSITION 6. With a force of given intensity all microts are equal in magnitude, no matter how far or near the state of rest.

Demonstration for propositions 5 *and* 6.

It is a very well-known fact, as has been proved by experiments and observation, that if a body be acted upon by any constant force for a time (t)—then the velocity (V) acquired by the body at the end of the time (t) is proportional to the time (t) : $V \propto t$; and, also, the space (s) passed by the body during the time (t) is proportional to the square of the time: $S \propto t^2$.

Now, these two proportionalities cannot very well be accounted for otherwise than on the assumption, that all the microfs and all the microts for a given force are equal among themselves.

It is evident, that if the accretions of velocity be equal among themselves, and that if they occur at regular intervals of time—that, under such a supposition, the final velocity must be proportional to the time.

Also, if the space passed over during the first interval be taken as unity, then, under the above supposition, the space passed during the second interval will be equal to (2); that passed during the third equal to (3); that passed in the fourth equal to (4). In general, the space passed in the (nth) interval will be equal to (n).

Now, supposing the time (t) to consist of (n) such intervals (microts), then, the space (s) passed over during the time (t) will be equal to $1 + 2 + 3 + 4 \ldots + n$. Sum up the series and we find that the entire space (s) will be equal to $(1 + n)\frac{n}{2}$, that is, $S = (1 + n)\frac{n}{2}$. Now, since (n) is a very large number for any sensible time, the unit in the brackets may be neglected. So that $S = \frac{n^2}{2}$ or $S \propto n^2$. But since (n) represents the number of intervals of time (microts) of which the time (t) consists, it is evident that if S be proportional to n^2, it must also be proportional to (t^2); $S \propto t^2$.

But on no other supposition can the two proportionalities, $V \propto t$ and $S \propto t^2$, be accounted for.

We have thus proven that with a force of *given intensity* both the microfs and the microts must be of equal magnitude.

Now, since the microfs do not vary with the intensity of the force (prop. 4 and corollaries), it follaws that *all* microfs, no matter what the intensity of the force and no matter, also, whether far or near the state of rest—are of equal magnitude. Q. E. D.

PROPOSITION 7. The magnitudes of the microts varies with the intensity of the force, being inversely proportional to the force producing the motion.

Demonstration. Since we have proved above that each and every microd and microf is a fixed and determined quantity, and is independent of the magnitude of the force producing it, the question arises, how does it come to pass that forces of different magnitudes operating through equal times produce velocities proportional to those forces? There is no other answer possible but that a larger force produces each and every microf in a shorter time than that which a smaller force requires for producing the microfs and these times are inversely proportional to the forces; for on this supposition the number of microfs produced by a larger force in a certain definite time will be larger than the number of microfs produced in the same time by a smaller force, and the two numbers will be directly proportional to the forces. And this will account for the proportionality of velocities to forces.

PROPOSITION 8. Preliminary Discussion.

We have incidentally remarked above, in corollary 2nd, to proposition 4, that although the magnitude of each microf and microd, no matter whether it be the 1st, the 10th, or the 100th, or, in general, no matter how far removed from the state of rest—must for all (and any) force, be considered as fixed and determined quantities, each for itself; that, although this is the case, we cannot on this account conclude that all *microds* (and microfs) are equal among themselves, "for it has not been proven that the resistance of inertia of the 2nd state of matter is equal in magnitude to the resistance of inertia at rest; nor that that of the third state, fourth, etc., are equal to that of the second or to any other state." (Corollary 2, Prop. 4).

Newton and his followers have, indeed, assumed that the force of inertia, that is to say the force of resistance of a body (of unit mass) to a change of state—that this force is invariably one and the same in magnitude, no matter what the state of the body offering the resistance to a change of that state; whether it be that of rest or of motion with *this* or with *that* velocity.

But, I say, that this assumption is entirely arbitrary, and has not even any logical ground to rest upon. Indeed, apriorily it would seem that the reverse of Newton's assumption should be true; that is to say, that the magnitude of the force of inertia should vary with the state of the body. Since inertia is a force of resistance which a body *in some state* offers to anything that tends to change that *state*—it is no more than reasonable to aproirily conclude that the magnitude of the resisting force should be different in different states. Nay, more, we may aproirily even determine the rule of variation of the force of inertia by reasoning thus: since the change of states of a body is from motion with one velocity to motion with another velocity, we, therefore, conclude that what costitutes *a state of a body* is the particular velocity with which it happens to move, and it might reasonably be said, that, when a body changes its velocity, it changes its state; and that, when the velocity is increased by the change, the change of states is from a lower to a higher state; and when, on the other hand, the velocity is diminished by the change, the change of states is from a higher to a lower state.

This being so, it is reasonable to conclude that the magnitude of the force of inertia, which is a resistance to a change from a particular state, that is, from a particular velocity, should correspond to that state, or to that velocity: that is to say, that it, the inertia, should be greater with a greater velocity and less with lesser velocity of the moving body. In other words, the resistance of inertia ought to be proportional to the velocity of the moving body.

For, the velocity, that is, the state of the body, is really the cause of resistance to a change of that state; the inertia, therefore, is in reality the effect or property of the velocity; and effects or properties must be proportional to their causes.

At least, I say, that our assumption that inertia varies with the velocity of the body and is proportional to it, is as much entitled to consideration as the assumption of Newton; and if, in applying the two assumptions to the phenomena of nature, we should find that our assumption offers a better and more satisfactory explanation of those phenomena, than the assumption of Newton, then, I say, it is our duty, nay, we are compelled, at once to reject Newton's assumption and adhere to *our* assumption as to the true representation of reality: for the voice of nature ought to be superior to that of any authority.

But (I say it unhesitatingly and convincedly) such is the case. It, our assumption, once accepted, at once renders the laws of the phenomena of motion, force, and energy clear and transparent, and they follow from it naturally and easily as a natural consequence; all darkness and confusion, all the difficulties enumerated in the two preceding sections and many more, are at once swept away by the mighty force of this eternal truth. More, this truth, once accepted, helps us to a clear and comprehensive understanding of many phenomena of nature, which are at present considered to be either mysterious or are eexplained in a way akin to the mythology of the ancients: such explanations, in effect, generally beginning with "once upon a time," like a fairy tale. More still, the acceptation of this truth gradually leads us to new views of the whole universe, of its working, construction, and fate; views more in accordance with reason and more applicable to a universe, as a universe, and, also, more befitting it as an effect of the Eternal Infinite First Cause—more, I say, than the views now prevalent and called "scientific."

All these favorable claims for the supposiiton that inertia is proportional to velocity will be made good, I hope, as we proceed along with this work. But in the meantine we will proceed to prove it rigorously according to the method followed in this section.

PROPOSITION 8. The microds vary in magnitude with, and are proportional to, the serial number of the state of the body. Thus, if you take the magnitude of the microd of the first state,, that of rest, as unity, then, the microd of the second state, that following the first microf, will be *two;* the microd of the third state, that following the second microf, will be *three;* the microd of the fourth state, that following the third microf, will be *four;* in general, the microd of the nth state, that following the (n—1) microf, will be *n.* Or if, because the best measure of force and energy is by velocity (Sect. 1, Book 1)—if, on this account, you decide to take the 1st microf (for a unit of mass) as the measure of the 1st microd, then, the magnitude of the 2nd microd will be equal to two microfs; that of the third, to three microfs; that of the fourth, to four microfs; and, in general, the measure of the nth microd will be equal to (n) microfs, or to the velocity of the body at the end (nth) state, or simply to the velocity of the body.

In plain language this proposition means, that the amount of energy required to produce the least possible change of velocity in a body moving with a certain velocity, is equal (or at least proportional), to the velocity of the moving body (for a unit of mass).

(That there is a least possible velocity, and, consequently, "a least possible change of velocity," we have proved above.)

Demonstration. Experiments and observations have taught, that the energy of a body moving with a certain velocity (V)—which energy is, of course, equal in amount to the amount of force exerted on the body for producing the velocity (V)*—that the amount of energy is proportional (for a unit of mass), to the square of the velocity; and that, if we choose to meas-

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***See Remark, Prop. 2.**

ure energy by velocity, it is equal to half of the square of the velocity $= \frac{V^2}{2}$. This is the physicial fact.

Now, suppose a body of unit mass, moving with a velocity (V), to be acted upon by any force for such a small time, that the resulting increment of velocity in the moving body be the least possible, that is to say, a microf (m'f), then, of course, will the increment of energy in the moving body be equal to a microd (m'd), that is to say, the increment of energy will be equal to the particular microd of the body in the particular state of velocity (V).

Now denoting the original energy of the moving body by E, its new total energy by E_1, its new velocity by V_1, the increment of velocity by (m'f); the increment of energy $m'd_v$* then, we get the following equations:—

$$V_1 = V + m'f \qquad (1)$$

$$E = \frac{V^2}{2} \qquad (2)$$

$$E_1 = \frac{V_1^2}{2} \qquad (3)$$

$$E_1 = E + m'd_v \qquad (4)$$

Subtracting equation (2) from equation (3) we get

$$E_1 - E = \frac{V_1^2 - V^2}{2} = \frac{(V_1 + V)(V_1 - V)}{2} \qquad (5)$$

Substituting in the last equation for E_1 and V_1 their values, as given by equations (4) and (1) we get

$$m'd_v = \frac{(2V + m'f)m'f}{2} \qquad (6)$$

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*Note: $m'd_v$ means the microd of the V state of the body; taking V as representing the number of microfs of which the velocity of the moving body is composed.

But since m'f is infinitely small, as compared with 2V, the (m'f) within the brackets in equation (6), may be neglected and we therefore get

$$m'd_v = V \times m'f \qquad (7)$$

Now, if we take (m'f) as the unit of velocity, then V will be equal to the serial number of the state of the body, and

$$m'd_v = V \times 1 = V; \qquad (8)$$

that is to say that the microd of the state V is equal to the velocity V. Q. E. D.

Corollary. Since, by corollary (1) proposition (1), the amount of resistance overcome bears a constant ratio to the amount of energy required to overcome that resistance, we, at once, infer from equation (7) $m'd \quad = V \times m'f$—that the resistance of inertia of a body moving with any velocity V, which amount of inertial resistance must always be equal to $m'd_v$—that this amount of inertial resistance must be proportional to Vm'f; or, since the microf (m'f) is a constant quantity, *inertia must be proportional to (V), the velocity.*

In point of fact the statements ought to be reversed, thus: Because inertia is proportional to the velocity of the moving body, this is why the microds are proportional also to the velocity of the moving body. It is only for the sake of argument that we reversed this statement.

Scholium 1. This last proposition and its corollary at once do away with all the difficulties enumerated in the last section; for, it at once shows why the measure of energy is by half the square of velocity.

If you take the first (or any) microf as the measure of the first microd (which is the least possible microd), then will the second microd be equal to two microfs, the third to three microfs; in general, the nth microd will be equal to (n) microfs. Put this statement into a series, thus:

$$\begin{aligned} m'd_1 &= 1 \text{ microf} = m'f \\ m'd_2 &= 2 \text{ micros} = 2\ m'f \\ m'd_3 &= 3 \text{ microfs} = 3\ m'f \\ \ldots\ldots &= \ldots\ldots = \ldots\ldots \\ \ldots\ldots &= \ldots\ldots = \ldots\ldots \\ m'd_n &= (n) \text{ microfs} = (n)\ m'f \end{aligned}$$

Sum up the series and it will be found that

$$m'd_1+m'd_2+m'd_3+\cdots\cdots+m'd_n=(1+2+3+4 \cdots+n)m'f=(1+n)\frac{n}{2}(m'f) \qquad (9)$$

But, since *one* is infinitely small, as compared with (n) for any sensible velocity, the (1) within the brackets in equation (9) may be omitted, and we get

$$m'd_1+m'd_2+\cdots\cdots+m'd_n=\frac{n^2}{2}\times m'f \qquad (10)$$

Now, if you take (m'f) as unity of velocity, you may put in equation (10) ($m'f^2$) instead of (m'f), for $1^2 = 1$ and we get

$$m'd_1+m'd_2+m'd_3+\cdots\cdots+m'd_n=\frac{(n^2).(m'f)^2}{2}=\frac{(nm'f)^2}{2}$$

But n × m'f is equal to the velocity of the body (V): $n \times m'f = V$. Substitute V instead of n × m'f in equation (11), and we get

$$m'd_1+m'd_2+m'd_3+\ldots+m'd_n=\frac{V^2}{2} \quad (12)$$

But $m'd_1 + m'd_2 + m'd_3 + \ldots + m'd_n$, being the sum of resistances overcome, as well as the sum of the amounts of force exerted for producing the velocity (V), represents also (remark to prop. 2) the whole amount of energy possessed by the body, when moving with the velocity V. Substitute, then, (E) (energy) instead of

$(m'd_1+m'd_2+m'd_3+\ldots+m'd_n)$ in equation (12),

and we get

$$E \text{ (energy)} = \frac{V^2}{2} \quad (13)$$

Q. E. D.

Scholium 2. It might be asked—if the resistance of inertia increases with the increase of velocity, how is it, then, that one and the same force, operating for one and the same interval of time (since all microts are equal for a given force, prop. 7), can overcome different resistances, varying in magnitude as the velocity of the body? In other words, according to our exposition as given above, during the first microt the force overcomes a resistance equal to one microf; during the second microt it overcomes a resistance equal to two microfs; during the nth microt it overcomes a resistance of (n) microfs. Now, since all microts for a given force are equal, how can this be explained?

To answer this, let us first of all remark that the "forces" we were dealing with throughout all these propositions refer to the inexhaustible, unlimited, and constant "forces"; that is to say, gravitations of different intensities; for, as we proved in Sect. 1, Book I., there is only one inexhaustible and unlimited and constant force, and that is gravitation; and when we speak of constant "forces," we, necessarily, mean gravitations of different intesities. We have also proved in Art 9, Sect. 1, Book I., that, with gravitation, the quantity of matter, acted upon by a gravitating force, makes no difference in the effect produced; the effect being the same, whether the quantity of matter be large or small.

Now, since matter divested of resistance (inertia) cannot be acted upon at all (prop. 1); and it is only by virtue of the force of inertia that matter becomes a something—therefore, I look upon the quantity of inertia a body possesses as upon a factor determining the quantity of its mass, as far as effects are concerned. I mean that a body of unit quantity of matter, but possessing (n) units of inertia, is looked upon by me as equivalent to a body of (n) units of matter, possessing a unit of inertia (as far as effects are concerned). Multiply the inertia of a body and you multiply its mass, as far as effects are concerned. Force acts not upon the matter, but upon the inertia it is invested with. The unit of inertia, since ineertia is measured by velocity, is the microf; and in a body invested with "n" microfs, force acts upon each and every microf, separately and independently of the others, as though it were alone in existence and the others did not exist; and, therefore, the force produces during a microt always the same effect as upon a body at rest, that is to say, invested with a unit of inertia. Just as the multiplication of the quantity of units of matter makes no difference in the effect produced by the force, and the force acts upon each unit of matter as though the others did not exist—just so the multiplication of the units of inertia (the microfs), makes no difference in the effect produced; and the force acts upon each, as though the others did not exist.

Remark. Let me add here that this peculiarity of gravitation is a very important peculiarity to be added to those already enumerated in Sect. I., Book I. It at once distinguishes the "constancy" of gravitation from other so-called "constant" forces. The force of a running stream, the force of the wind, the force of an electric current may be "constant" for some duration.

But just mark that these finite "constant" forces cannot and do not produce double the velocity in double the time, or treble velocity in treble time, or, in general, a velocity directly proportional to the time of action; because those finite "constant" forces do not act on a body in motion as though it were at rest, as is the case with gravitation. Let a constant current of electricity be employed to give motion to a train, for instance, devoid of all friction; then, if it communicate to the train a velocity of thirty-two feet in the first second, the velocity found to have been communicated to it at the end of two seconds will not be $2 \times 32 = 64$, but $\sqrt{2} \times 32$; at the end of three seconds the velocity will not be $3 \times 32 = 96$, but it will be $\sqrt{3} \times 32$, and so on. Why so? Because with the increase of motion the resistaance of the train to a further increase of motion becomes greater and greater, while the energy the current is capable of yielding in one second, is constant.

With grativation it is altogether different. Gravitation disregards the motion of a body, so to speak, and acts on it as though it were at rest. For the energy of gravitation is limited only by the capacity of the body acted upon, that is, by the resistance it offers.

In order to prove the assertion about a "constant," finite force made above, let E be the energy any "constant," finite force yields in one second, and let V_1 be the velocity it produces during the first second in a body at rest; so that $E = \frac{V_1^2}{2}$; at the end of "N" seconds the energy spent by the constant, finite force will be $NE = \frac{NV_1^2}{2}$; and the velocity V_n * will therefore be such, that

$$\frac{V_n^2}{2} = NE = \frac{NV_1^2}{2}; \text{ or } V_n = V_1\sqrt{N}$$

that is to say that the velocity at the end of the Nth second equals $\sqrt{N}$ times the velocity of the first second.

PROPOSITION 9. A body moving with any velocity, if it act on another body by virtue of its molar motion, acts so by virtue of its inertia; and therefore does the measure of its inertia constitute also the measure of its action.

Demonstration. This seems to me to be self-evident. Since the only force molar motion confers upon a body is that of an increase of its power of resistance to a change of state, that is to say, an increase of its inertia; therefore, if the body act by virtue of that molar motion, it must necessarily act by virtue of that only force which the molar motion confers upon it; that is to say, by virtue of its inertia; and if it acts by virtue of inertia, the effect it produces must be proportional to the inertia; for effects must always be proportional to their causes. Therefore I say, that the measure of its inertia constitutes also the measure of its action—the measure of its effects.

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*Note: By V_n is meant here the velocity the body will have at the end of "N" seconds.

Corollary 1. Since the measure of inertia for a unit of quantity of matter is constituted by its velocity, which denote by V; and since what is true of the unit must, for many units, be multipled by the number of the *many*—therefore, must the measure of inertia of M units of matter moving with a velocity V be by the product MV.

And we at once see that MV=the momentum of physicists—is nothing else but the measure of inertia of the body M moving with a velocity V; and that the measure of its action, as far as molar motion is concerned, is also by the same MV—the momentum.

Scholium. This proposition and its corollary, at once remove all the perplexities enumerated in Sect. I., Book II. It, at once, clearly shows that the "force" of physicists, which is measured by mass and velocity—momentum—is nothing else but the force of inertia. And, because inertia has reference only to molar motion, this is why the meaure of "force" by momentum is only applicable within the sphere of molar motion, exclusively, as we have pointed out in Sect. I., Book II.

Let us enter into some detail. The measure of "force" by momentum regulates, principally, the law of action and reaction. Let us, therefore, consider several cases of action and reaction, and see whether or not our above proposition satisfactorily explains why momentum, or, as we call it, the measure of inertia, should constitute the measure of "action and reaction are equal and opposite."

(1.) First, let us take the case of the collision of bodies. Suppose a body "A" of mass "M," moving with a velocity "V," to strike in its course another body "B" of mass "M_1," at rest. Body "B," because of its inertia, offers a hindrance to the further progress of body "A." Body "A," in its turn, strives, by virtue of its inertia, to maintain that motion, and so imparts part of that motion to body "B." But what body "A" imparts to body "B" is *motion,* not energy; for it is only the *motion* of body "B" that renders the continuance of the motion of body "A" possible; and, therefore, for every minute quantity of motion gained by body "B," there must be a corresponding and equal quantity of motion lost by body "A." And that is what is really meant by "action and reaction are equal and opposite," as applied to this case. But the quantity of motion is nothing but velocity multiplied by mass; that is to say, it is nothing else but the quantity of inertia or momentum. Motion, as contemplated with reference to the state of the moving body, is nothing else but inertia; and a quantity of motion is nothing else but a quantity of inertia. By motion, as referred to the state of the moving body, we mean velocity; but velocity is the measure of inertia.

(2.) Let us now take the case of a discharged rifle. The rifle ball is propelled with a certain velocity, and the rifle stock suffers a certain recoil, called the *reaction;* and it is found that the velocity of the ball multiplied by its mass is equal to the velocity of the stock multiplied by its mass; and it is, therefore, said that action and reaction, in this case also, are equal and opposite.

Let us, then, see whether our proposition explains this case also. Suppose, for simplicity, the mass of the ball to be one unit, that of the stock to be "M" such units; then, is the inertia of the ball at rest equal to one microf, and that of the stock equal to "M" microfs. But "M" is by supposition more than "one." The inertial resistance of the ball is, therefore, smaller than that of the stock. Now, when the force—the pressure of the gases resulting from the explosion of the gun powder—begins to act, it acts, as it must, on the body with the least resistance; that is to say, on the ball, but not on the stock, for the stock offers greater resistance to a change of state than the ball at the commencement of action. But after the force has imparted to the ball a velocity of 2M microfs the resistance of the ball becomes larger than that of the stock, and then, only then, the force begins to act on the stock; and will produce in the stock one microf, so that the resistance of the stock will also be equal to 2M. The resistance of the ball to further action will again then be the least of the two, and consequently all action on the stock will cease, until the ball acquires a velocity equal to 3M microfs and its resistance becomes larger than that of the stock, and a microf will then be produced in the stock. This sequence of events will take place many times until the force is exhausted; and the result will be "action and reaction are equal and opposite," nearly.

I say *nearly,* for from what has been said it is evident that there must not necessarily be an exact equality between the action and the reaction. Thus, suppose this sequence of events to have taken place (N-1) times, so that the resistance of the ball and the resistance of the stock are each equal to MN microfs. Now, suppose the force to act further on the ball, and to produce in it an additional velocity equal to "M" microfs; and at this instant suppose the force to get exhausted so that no further action on the stock is possible—then will the momentum of the ball be equal to MN + M, while the momenetum of the stock will remain equal to MN: there will, then, be a difference of the two momenta equal to "M" microfs.

Indeed, if "M" be very large, that is to say, that the proportion between the two masses is very large, and the force be comparatively small, so that it gets exhausted at the end of its first action on the ball, that is to say, after it produces a velocity in the ball equal to 2M microfs or less—then, no action will take place on the stock at all; that is to say, there will be no reaction at all.*

(The earth's mass, as compared to the amount of matter ejected on each volcanic explosion is almost infinitely large; and this circumstance will perhaps explain why the earth suffers no recoil on volcanic explosions. The assertion that it does suffer a minute recoil besides that it is contrary to the experience of mankind, for at places, at considerable distance from the volcano, no shock at all can be felt or even demonstrated—besides this, will involve us in great difficulty, for the velocity of the earth's revolution around the sun ought, for this reason, be disturbed.)

Ordinarily, however, the difference of the moment, 2M microfs, which is the maximum difference possible—is a hardly sensible quantity, and the law of action and reaction holds good.

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*Note: This may possibly suggest a mode of experimentally determining the actual magnitude of a microf. For, suppose that we contrive to be able to register the slightest motion of the stock, then we may at pleasure gradually increase the mass of the stock and diminish the amount of force until the registered motion of the stock will be equal to zero. The velocity of the ball divided by 2M will, then, be equal to a microf.

Briefly stated, our explanation why the law of action and reaction should be measured by monmenta is this: It is so, because the resistance on the two sides of the force must be equal; for if not, the force will act on one side only, on that, namely, where the resistance is the least and not on the other. But since the force acts in both directions, the resistances must be equal; but as resistance of inertia is measured by what is called momentum, as shown above, it is, therefore, that the momenta on both sides are equal.

(3.) The case of a spring drawing together two bodies of different masses. The reasoning in this case is exactly similar to that of the above case.

(4.) In the case of attraction between two bodies due to grativation, we need not, necessarily, resort to the law of action and reaction in order to explain the equality of the momenta on both sides. For this equality of momenta follows simply from the fact that the intensity of gravitation is proportional to the mass exerting it, and that the magnitude of the mass acted upon makes no difference in the effect produced as was proven in Sect. I., Book I. Thus, suppose a body A of N units of mass and a body B of M units of mass to be mutually acted upon by the gravitation of their masses. The intensity of the gravitation of B as compared to that of body A is proportional to their masses.

$$\frac{\text{Intensity of B}}{\text{Intensity of A}} = \frac{M}{N} \quad (1)$$

But the velocities produced by constant forces in equal times are proportional to their intensities, therefore, the velocity of A, which is produced by the gravitational force of B, is to the velocity of B, which is produced by the gravational force of A, as the intensity of the gravitation of B is to that of A.

$$\frac{\text{Vel. A}}{\text{Vel. B}} = \frac{\text{Intensity B}}{\text{Intensity A}} \qquad (2)$$

$$\text{But} \quad \frac{\text{Intensity B}}{\text{Intensity A}} = \frac{\text{M}}{\text{N}} \qquad (1)$$

$$\text{Therefore} \quad \frac{\text{Vel. A}}{\text{Vel. B}} = \frac{\text{M}}{\text{N}} \qquad (3)$$

or velocity of A × N = velocity of B × M.
or momentum of A = momentum of B.

I cannot, and I consider it a sacred duty not to, refrain from adding a few more remarks on this for centuries sanctioned third law of Newton—"action and reaction," etc. These remarks will probably not be very palatable to my reader, nor are they very palatable to myself, appreciating as I do the humbleness of my position and the greatness of the one who promulgated this law and of those who, for centuries after him, upheld it. But express my conviction I must, come what will. And my humble conviction, briefly stated, is this:

This law, although nearly true, as far as its application to the phenomena of nature is concerned, has, in the way it is stated, no physical meaning, and is perhaps not always exactly true. Yet from childhood I often wondered at this *queer* law. What do you actually mean by reaction? and in virtue of what should a body react? A body, possessing some energy, acts upon another body which does not possess energy. You say that the body which does not possess energy, and is therefore not active, nevertheless, reacts upon that other body. What do you mean by react? and in virtue of what does it react? Is reaction a thing, a property inherent in bodies? Is it a dormant power in a body that becomes active or rather reactive only when the body is acted upon? A body at rest is struck by a body in motion, and you say that the body at rest which possesses no motion *reacts* and produces in the other body a *motion* in an opposite direction. It costs very little effort to say it; but all the efforts I am capable of exerting fail to make me clearly comprehend it. Wherefrom did the body at rest get motion to communicate to the other body in an opposite direction?

"If you press a stone with your finger," says the great Newton, "the finger is also pressed by the stone." Yes. But the finger is not the active agent. For the stone, the finger, and your hand, and perhaps also your arm and forearm are all *pressed* by the contraction of a certain muscle, attached with one extremity to the body and with the other to the arm, the biceps, for instance; and the finger is only squeezed, but not *pressed* or acted upon by the stone.

In the second example of Newton, that of a horse drawing a stone tied to a rope—the horse, that is to say, the body of the horse is not the active agent. The body of the horse, the rope, and the stone are all drawn by the contraction of certain muscles, attached with one extremity to the legs of the horse, which rest on the ground and are therefore *fixed*, and by the other extremity to the body of the horse. In fact, the horse's body, the rope, and the stone must all be looked upon as one body, while the legs and the earth must be looked upon as another body; and the contracting muscles, as the spring drawing the two together; and because the legs are *fixed* by the earth and cannot be moved, therefore, the horse's body, the rope, and the stone move, while the legs remain *fixed*.

This example, then, so far from showing action and reaction, actually shows the opposite—that there is only action, and no reaction at all, unless you maintain that the legs and the earth upon which they rest actually move backward under the horse, which, to my mind, is a very extravagant idea; since such a motion of the earth, no matter how slight, will affect the length of the day, if the horse move in a west and east direction, or it will affect the position of the earth's axis, if the horse move in a north-south direction.

In fact, if we carefully analyse all possible cases of interaction between two bodies as far as molar motion is concerned, we will find them to correspond to either of the four examples given above:—

(1) Collision.
(2) Discharged rifle.
(3) The spring between two bodies.
(4) Attraction: gravitation or other attractions (or repulsion).

In the first class there is simply a communication of *motion* from a body which has more of it to a body which has less of it, but there is no necessity of introducing any "reaction" between the bodies.

In the second and third class, to which all illustrations of action and reaction belong, such as the horse drawing a stone, or the man towing a boat, etc. —in these two classes there is positively no *reaction,* but all is plain action; for the force acting between the two bodies acts on both of them. If a stretched spring draws two bodies together, it positively *acts* on both bodies; and I fail to see why the dragging of one body should be entitled to the term *action,* and the dragging of the other to *reaction.*

In the fourth class there is no necessity of *reaction* again, as we have clearly shown above.

Consider also the extravagant notions in which we become involved through the blind and unintelligible strict law of "reaction." If a man throws a stone upward, he not only throws the stone, but he actually, though unconsciously, pushes, with his feet, the earth out of its orbit; more, he causes the earth after it has been so pushed out, to fly upward and follow the stone, while the stone is in the air. That is what physicists of the nineteenth and twentieth century really believe, and what they must believe according to what they accept as an inflexible law of nature—"action and reaction are equal and opposite." Nature becomes, according to this law, extremely *shaky.* Not the slightest motion can possibly take place in nature without there being somewhere a counter motion.

Another consequence of the law of "action and reaction" is, that all *actions* of nature must be looked upon as mutual; and when we say that one body acts upon the other body, meaning thereby that we conceive the first body as the active agent, and the second as the passive agent, merely undergoing the effect of the action—then, we, according to the law of "action and reaction," commit an unpardonable error: there is no *activity* and no passivity in nature, according to this law; all is action and counter action; all is passion and counter passion. In a word, nature, according to this law, as understood by physicists, becomes cloudy and hazy, and not to be spoken of without falling into error; that is to say, that nature, as it is in reality, does not correspond to the laws of mind.

Now, consider, on the other hand, the simplicity and the conformity with reason of our assertions with respect to the "equality of momenta on both sides," as well as in many other respects.

We first assume in our first p[illegible] rollaries the equality between the amount of force exerted and the resistance overcome—an almost apriori truth. We next infer that inertia must be conceived as a resistance. Next, we attempt to show on apriori grounds, that the force inertia must be proportional to the velocity of the body. We, then, demonstrate our last assertion aposteriorily in proposition 8. And these simple, almost childish truths, coupled with another similar truth, that a force when acting between two bodies will first act on the body with the least resistance—these simple truths make it at once apparent why the momenta are "equal on both sides." The "equality of momenta on both sides" follows naturally and logically from these simple truths as day follows night.

Our view of gravitation that it is not a force acting between two bodies, like a muscle acting between two bones, but that each body acts separately and independently upon the other by virtue of the force of gravitation inherent in its own mass, and that this is why the momenta on both sides must be equal—this view of the matter seems to me to be more acceptable to the mind than the mystical mutual "action and reaction."

In cases of collision we have pointed out there is only a communication of *motion*, just as heat is communicated from one body to another body.

Further, our theory about microfs (Prop. 2), makes it at once apparent why, when the proportion between the masses of the bodies is very large, and the acting force comparatively small—why no action at all should take place in the larger body, as we have explained above.

[illegible] crots, explains why, when a stone is thrown upward, the earth does not fly after it; for the magnitude of the microt for the intensity of the stone's gravitation is considerable; at least it is more than the time the stone remains in the air.

These considerations and others almost compel us to substitute for the law of "action equal and opposite to reaction" the more simple and at the same time the more comprehensive law of *Action Equal to Resistance Overcome.*

This simple law at once explains all the prenomena of molar motion and of energy. This is the simplest, the grandest, and sublimest law of nature.

I am at the end of my remarks, and will close our 9th proposition with an earnest request from the reader to re-read carefully Sect 1 and 2 of Book II., and so reconvince himself that these few propositions entirely, most satisfactorily and naturally, do away with all the difficulties enumerated there. I must warn the reader, however, against confusion arising from the loose and indefinite meaning of the term "force" used in those two sections, for which I ask the reader's indulgence. I could not help using "force" in different meaning in different places without inventing a new term, which I did not wish to do.

Although our task is ended as far as it goes, still I will add the following two propositions, because they relate to gravitation, of which we principally deal in this volume.

PROPOSITION 10. If two forces, gravitational or others, of different intensities, act in opposite direction on a body, then is the microt of the resulting motion equal to the microt of the smaller force multiplied by that of the larger force and divided by the difference between the microts. Thus, denote the large force by F, the smaller force by F_1, the microt of the larger force by (M), and the microt of the smaller force by (M_1), and the microt of the resulting motion by (M_2); then is

$$M_2 = \frac{MM_1}{M_1 - M}$$

Demonstration. This is evident. Since the microts are inversely proportional to the intensity of the forces, we, therefore, have

$$\frac{F}{F_1} = \frac{M_1}{M} \qquad (1)$$

$$\text{and} \qquad \frac{F - F_1}{F_1} = \frac{M_1 - M}{M} \qquad (2)$$

$$\text{or} \qquad \frac{F_1}{F - F_1} = \frac{M}{M_1 - M} \qquad (3)$$

But $F\text{-}F_1$ is the force by which the motion in the instance given above results; and, therefore, its microt or (M_2) divided by the microt of the force F_1 must be equal to

$$\frac{F_1}{F - F_1}$$

$$\text{or} \qquad \frac{M_2}{M_1} = \frac{F_1}{F - F_1} \qquad (4)$$

But from equat. (3) we have $\frac{F_1}{F—F_1}=\frac{M}{M_1—M}$ Substitute this value of $\frac{F_1}{F—F_1}$ in equat. (4), and we get

$$\frac{M_2}{M_1}=\frac{M}{M_1—M} \qquad (5)$$

or finally $M_2=\frac{MM_1}{M_1—M}$

Q. E. D.

Scholium. From this proposition it is seen that the smaller the difference between the two forces, and consequently between their microts, the larger is the microt of the resulting action; so that the magnitude of the microt in such cases may extend to hours, days, or even years. This circumstance will explain the following otherwise inexplicable fact in physics:

"Tenacity is the resistance which bodies oppose to traction. It is determined in different bodies by forming them into cylindrical or prismatic wires and ascertaining the weight necessary to break them. . . . Tenacity diminishes with the *duration* of traction. A small force continuously applied for a long time will often break a wire which would not at once be broken by a larger weight."

(Ganot's Physics, by Atkinson, 7th Edition.)

Now, two forces are concerned in this experiment. On the one hand, we have gravitation, tending to produce downward motion in the weight, and, on the other hand, the elastic force of the wire tending to raise the weight. And it seems to be inexplicable, why a weight that cannot break the wire at once, should, after some duration, succeed in doing so. Either gravitation is the stronger of the two, or the elasticity is the stronger. If gravitation be the stronger, then why does not the downward motion of the weight take place at once? On the other hand, if elasticity be the stronger, why does the downward motion and the breakage of the wire take place altogether?

But our proposition offers a ready explanation of this fact: gravitation is very slightly the stronger. Still, downward motion does not take place at once, because the least possible motion is that with a velocity of one microf; and for the production of a microf, *duration* is a necessity—a microt; and in this case, the unbalanced force being very small, the microt is very large, and when it expires, be it a day or a year, a microf is completed, and downward motion takes place.

This fact offers very good experimental proof of proposition 2; for, if we accept the opinion of physicists, that a force, no matter how small, produces motion at once in a mass, no matter how large—if we accept this opinion, I do not see how this fact can be explained.

PROPOSITION 11. Gravitation can overcome the resistance of inertia only; but it cannot overcome any other resistance; all other resistances can only be overcome by what I call finite forces, such as motion, heat, light, electricity, magnetism, and chemical affinity.

And, further, gravitation overcomes the inertia, not only of the body acted upon, but also the inertia of other bodies not directly acted upon, but in some way connected with the body directly acted upon and impeding its motion.

In such cases, is the microt of the resulting action to the microt of a free body acted upon by the same gravitational intensity—inversely proportional as the mass of the body directly acted upon is to the sum of its mass with that of the body or bodies impeding its motion, but not directly acted upon.

Demonstration. There are only two kinds of resistances to the action of force. Firstly and primarily, the resistance of inertia; secondly, the resistance of cohesion. All other resistances, as friction, elasticity, tenacity, etc., are essentially cohesive. There is a vast difference between the two resisting forces, and it is this: The resistance of inertia has no maximum limit, because it increases with the action of the force, being, as it is, proportional to the velocity—while the resistance of cohesion is limited, being, as it is, fixed and determined, for each and every substance.

Now, I say, in the first part of this proposition, that gravitation, which is an unlimited force, acts and overcomes the unlimited resistance—inertia; but it does not act at all on, and, consequently, cannot overcome, the limited resistance—cohesion.

For, if you admit the opposite, then, if a weight be suspended from a rope, the weight, no matter how small, ought invariably and always break the rope, no matter how strong the rope might be, allowing sufficient time for gravitation to act. The only effect of the great resistance of the rope would be to lengthen the microt. And, in general, all solid substances in nature ought to have been crushed and pulverized by gravitation. Where a falling body overcomes cohesive resistance, as when a body slides downward on an inclined plane in opposition to friction, or when a large weight breaks a rope—it is really not gravitation that overcomes the cohesive resistance, but it is the *motion* of the body that overcomes the resistance, as may be gathered from our tenth proposition; and, if the cohesive resistance to motion be more than a microf, multiplied by the mass of the body acted upon by gravitation, then no motion will result, no matter how long gravitation continues to act upon the body.

This principle of nature is of great importance in the economy of nature, as well as in the science of mechanics; but, I am loath to discuss it here, and I rely upon the penetration of the reader: first, clearly to understand my meaning (for I feel that the exposition is rather obscure); and, secondly, to appreciate its wide and extensive application in nature and in science.

The latter part of our proposition finds its illustration and demonstration in the principle of Atwood's machine:—If two equal weights, each equal to "M," be attached to a cord thrown over a pulley, gravitation will have no effect on them. Now, if you add to one of the weights an *overweight* "m," the whole will begin to move ,and the acceleration of the whole wiil be found equal to $g\left(\frac{m}{m+2M}\right)$.

This shows us, firstly, that although the inertia of the two weights "M" is a resistance to the motion of the overweight "m," added from without, still, gravitation acts upon it. For, if gravitation does not act upon it, then, when a microf is produced in "m," its motion would be resisted, and no further action would take place, since there would be no change of state and no further resistance to be overcome. This is exactly the case with a weight suspended from a rope when the resistance of the rope is larger than one microf multiplied by the mass.

Secondly, it shows us that because the resistance of body "m" to the whole inertia is equal to $\frac{m}{m+2M}$ therefore, is the acceleration also equal to $g\frac{m}{m+2M}$

But acceleration is always inversely proportional to the microt, since a microf is a constant quantity. Hence our proposition.

APPENDIX.

I see fit to append a short sketch of our theory of force, energy, resistance, inertia, and the third law of Newton, omitting altogether the introduction of natural magnitude: microfs, microds, etc. Because the introduction of limited magnitudes in the domain of physics is an entirely new conception, it might be objected to by the conservative class of my readers. And, I say, that we assume and mostly prove:—

1. That without resistance to be overcome there can be no exertion of force, and, therefore, no action. (Proposition 1.)

2. That in consequence of the above, action is equal to the resistance overcome: that the amount of force exerted and the amount of energy conferred or destroyed bear a constant ratio to the resistance overcome. (Corollary to Proposition 1.)

3. That in consequence of the above, inertia must be conceived as a resistance.

4. That inertia varies with, and is proportional to, the velocity of the body.

This last we attempted to prove apriorily in the preliminary discussion to Proposition 8. If the reader is not satisfied with the apriori proof, he may consider it at present as purely assumptive; and, then, when we succeed in showing that this assumption clears away all difficulties in the conceptions of force, energy, and action and reaction, then, this is, in itself, the best possible proof that our assumption is perfectly true.

Now, I claim, that these four statements explain:

(a) Why the measure of force, as related to the law of action and reaction (Sect. I, Book II), is by mass and velocity.

(b) Why the measure of energy is by mass and half the equare of velocity. (Sect. II, Book II).

As to the former, it is evident that if a moving body act upon another body, tending to impede its motion and so lessen its velocity, it is evident, that the only force by which the first body acts, is that of inertia, or resistance to a change of state which the second body tends to produce in it. And, if it acts by inertia, the effects produced must be proportional to the inertia, for effects are proportional to their causes. And, since the measure of inertia is, according to our fourth assumption, by velocity, and, of course, by mass also, therefore, is the measure of the effects of inertia also by mass and velocity—momentum.

Further, if a force act between two bodies, the resistance of the two bodies must tend to be equal; for, if one body offer greater resistance and the other less, the force will produce motion only in that which offers less resistance; and will continue to act on it only till by increase of velocity its resistance becomes equal (or slightly greater than) the resistance of the other body. And, therefore, the resistances of both bodies must tend to be equal within narrow limits. But the resistance of inertia is measured by momentum, therefore must the momenta of the two bodies tend to be equal within narrow limits; and this is what must be understood by "action and reaction are equal and opposite."

As to the second, we will prove by the method of differential and integral calculus, that if our fourth assumption be true, and taking into consideration the assertion made in our second assumption, then must the measure of energy be by mass and half the square of velocity:

In order to determine the amount of energy a body moving with a velocity "V" possesses, it is only necessary to find the *whole* resistance overcome by the force in generating the *whole* of the velocity V from a state of rest; because energy is equal to the force exerted, and the force exerted is equal to the resistance overcome.

Denote the whole resistance overcome in generating the velocity V, in a unit of mass, by R:

$$\text{put } R = \Phi\, V \qquad (1)$$

Increase V by the infinitesimal quantity dv, then, we get

$$R+dR=\Phi\,(V+dv) \qquad (2)$$

Expand by Talyor's theorem, and we get

$$R+dR=\Phi\, V+f_1(v)dv+f_2(v)dv^2+f_3(v)dv^3 \;\;. \;\;. (3)$$

Where $f_1(v), f_2(v), f_3(v)$ denote respectively the 1st, the 2nd, the 3d . . . . differential coefficients. But $R=\Phi\, V$. . . . . . (1) We therefore get

$$dR=f_1(v)dv+f_2(v)dv^2+f_3(v)dv^3 \text{ etc.} \qquad (4)$$

Now let R_1 be the whole resistance overcome in generating in a unit of mass the velocity V_1

Put, as before, $R_1=\Phi\, V_1$ (5)

Increase, as before, V_1 by dv, and we get

$$R_1 + dR_1 = \Phi(V_1 + dv) \qquad (6)$$

Expand, as before, and we get

$$R_1 + dR_1 = \Phi V_1 + f_1(v_1)dv + f_2(v_1)dv^2 + f_3(v_1)dv^3 \qquad (7)$$

or reducing, as before:

$$dR_1 = f_1(v_1)dv + f_2(v_1)dv^2 + f_3(v_1)dv^3 \;\ldots \qquad (8)$$

Now divide equation 4 by equation 8, and we get

$$\frac{dR}{dR_1} = \frac{f_1(v)dv + f_2(v)dv^2 + f_3(v)dv^3 \;\ldots\ldots}{f_1(v_1)dv + f_2(v)dv^2 + f_3(v_1)dv^3 \;\ldots\ldots} \qquad (9)$$

Divide the numerator and the denominator of equation 9 by dv and we get

$$\frac{dR}{dR_1} = \frac{f_1(v) + f_2(v)dv + f_3(v)dv^2 \;\ldots\ldots}{f_1(v_1) + f_2(v_1)dv + f_3(v_1)dv^2 \;\ldots\ldots} \qquad (10)$$

In the limit dv becomes zero and we, therefore, get

$$\frac{dR}{dR_1} = \frac{f_1(v)}{f_1(v_1)} \qquad (11)$$

But $\frac{dR}{dR_1}$ must be equal to $\frac{V}{V_1}$,

because dR expresses the amount of resistance overcome in increasing the velocity from V to V+dv and in the limit it is, according to our fourth assumption, proportional to V; the same, of course is true of dR. Therefore,

$$\frac{dR}{dR_1} = \frac{V}{V_1} \qquad (12)$$

From equations 11 and 12 we get

$$\frac{f_1(v)}{f_1(V_1)} = \frac{V}{V_1} \qquad (13)$$

which is only possible when $f_1(v) = MV$ (14)

and . . . . $f_1(v_1) = MV_1$ (15)

"M" being an undetermined arbitrary constant.

But $f_1(v)$ is the first differential coefficient of R in terms of V, and is equal to $\frac{dR}{dv}$, therefore,

$$\frac{dR}{dv} = MV \qquad (16)$$

or $dR = MVdv$ (17)

Integrate $\int dR = \int MVdv + C$ (18)

or $R = \frac{MV^2}{2} + C$ (19)

But C must be equal to zero, because when V is zero R is also zero, therefore,

$$R = \frac{MV^2}{2} \qquad (20)$$

and M is only a constant of proportionality, showing that the ratio between the whole resistance overcome and the half square of velocity generated is constant. Thus $\frac{R}{\frac{V^2}{2}} = M$; and if we choose to measure resistance and energy by velocity, M becomes unity; therefore,

$$R = \frac{V^2}{2} \qquad (21)$$

But, since the energy conferred is equal to the resistance overcome, we may substitute E (energy) instead of R in equation (21), and we finally get

$$E = \frac{V^2}{2} \qquad (22)$$

Q. E. D.

BOOK III.

THE COMPOSITION OF FORCES

AND

THE CONSERVATION OF ENERGY

BOOK III. THE COMPOSITION OF FORCES AND THE CONSERVATION OF ENERGY.

PRELIMINARY REMARKS.

This subject is the last, in sequence, of Part I of this work. But I ought not to conceal from my reader that what comes here last is actually first in importance, and that it is only for the sake of system and argument that I wrote five sections in advance of it. All those five sections are only an introduction to this last subject. The reader was led by me through all that labyrinth of reasoning, in order to, as far as possible, purify his mind of some of the gravest scientific notions prevailing in this department of physics today. Notions that, because sanctioned by age and because enunciated and upheld by the greatest scientific minds of the last two centuries, have taken such firm roots in the mind of every scientist, that the mere questioning of them is likely to be considered as a great scientific crime—as a wilful and unwarranted commission of sacrilege. And the daring questioner is more than likely to suffer, what will hastily be considered, a just and fully deserved punishment—inattention, and more than contempt. To dare question such primary and grand principles of science, as the law of action and reaction, the grand and sacredly held generalization of conservation of energy; to question the measure of force and the law of inertia; to attempt a refutation of potential energy; to materialize energy and call it a *something,* not a capacity; to dare call gravitation infinite, eternal, and one. . . .

Is not such daring and such arrogance deserving of the severest infliction of the greatest possible punishment? Why! these principles are the boast of science, and their achievement costs us centuries of hard labor by the greatest men. True, no doubt; but the only possible defense the author of this can offer is—sincerity of purpose and the strongest possible conviction of the truth of his assertions.

Of course, within myself, I believe that my best possible defense lies in the arguments and reasonings written by me in the five preceding sections, and to be written in the following. But should I have erred, this avowal here of sincerity of purpose and strong conviction will serve a good purpose.

To return—I purposely conducted the reader through all that labyrinth of reasoning of the five preceding sections, in order to purge his mind of some of the firmly rooted and, to my mind, erroneous scientific principles, in order that he may be in some way prepared to enter into what I may call My Sanctuary—these our subsequent sections.

BOOK III. THE COMPOSITION OF FORCES AND THE CONSERVATION OF ENERGY.

SECT. I. THE COMPOSITION OF FORCES AND THE CONSERVATION OF ENERGY.

1. *The law of composition of forces stated*

Every beginner of the study of physics must have been greatly fascinated by the beauty and by the grandeur of that almost sublime geometrico-physical principle, known as the law of parallelogram of forces. Not only is it grand in its conception, I mean, logically considered, but as a law of nature and of science it stands first, as to its extent and import.

In science, it forms the basis of both static and dynamical mechanics and all their dependencies. Remove this law, and all statical, dynamical and hydrodynamical mechanics vanishes.

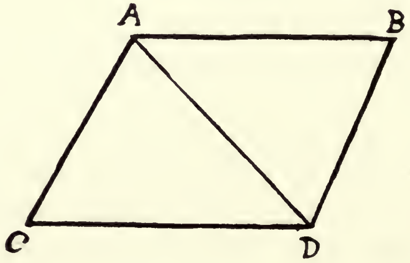
In nature, it is coextensive, as to its applicability, with the whole universe, for the motions of all heavenly bodies must be and are regulated according to this law; as it is to be inferred from the equalibility of areas described in equal times and a thousand other things. (See Newton's Principia, Book I, Sect. II., Prop. 1.).

Although hinted at by Plato, and subsequently clearly conceived by Galileo, it was first clearly and definitely stated to us, by the greatest man the English nation ever gave to the world—Sir Isaac Newton—in his first corollary to the laws of motion, which we will quote:

"A body by two forces conjoined will describe the diagonal of a parallelogram in the same time that it would describe the sides by those forces apart.

"If a body in a given time, by the force M (see figure below) impressed apart in place A should be carried with a uniform motion from A to B; and by the force N impressed apart in the same place should be carried from A to C; complete the parallelogram ABCD, and, by both forces acting together, it will in the same time be carried in the diagonal from A to D. For, since the force N acts in the direction of the line AC, parallel to BD, this force (by the second law) will not at all alter the velocity generated by the other force M, by which the body is carried toward the line BD. The body, therefore, will arrive at the line BD in the same time, whether the force N be impressed or not, and therefore at the end of that time, it will be found somewhere in the line BD. By the same argument, at the end of the same time it will be found somewhere in the line CD. Therefore, it will be found in the point D, where both lines meet. But it will move in a right line from A to D by law 1."

FIG 1.



Art. 2. *Remarks on the above.*

It is of great importance for us to notice, that the forces M and N, mentioned in Newton's first corollary, must have reference to what is generally called impulsive force, since, by supposition, the body is carried from A to B by M, and from A to C by N, "with a uniform motion"; and a "uniform motion" can result from an impulsive force only.

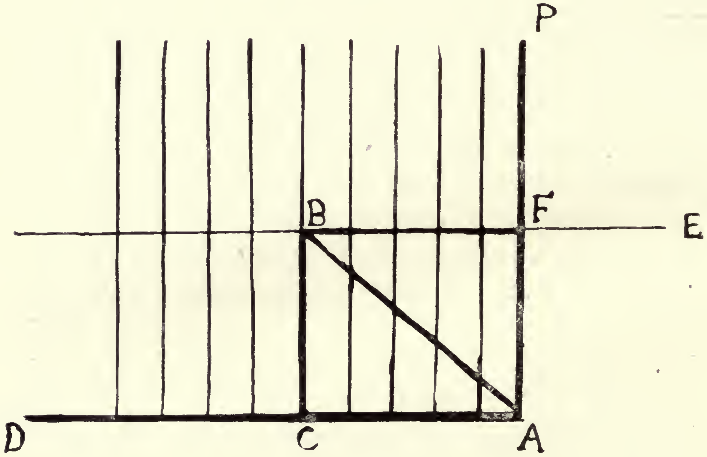
This shows us plainly that the measure of force by momentum produced, as enunciated in the 2d law of Newton, is meant by him not only as a measure of constant forces, or what I please to term inexhaustible forces—gravitations—but also as a measure of impulsive forces. That is to say, that momentum, as a measure of force, was intended by Newton not only as a measure of quality—intensity—but also as a measure of quantity. (See Arts. 14, 15, 16, Sect. I, Book I.)

All physicists have blindly and, I say, carelessly followed Newton in this, and have given the measure of an impulsive, exhaustible force by the momentum it produces; that is to say, that with the same body, impulsive forces are directly proportional to the velocities they produce—contrary, I am sure, to their own conviction and to the law of energy; for, according to the law of energy, impulsive forces, with the same body, are directly proportoinal to the square of the velocities they produce. (See Art. 8, Sect. I, Book II.)

Remark 2. Any number of parallel lines, whether in one plane or in many planes, are said to be in one direction. This is what is to be mathematically understood by direction.

Now, imagine a great number of parallel lines in any one plane, as in figure 2, and let a body be moving in that plane, from the point A, in the line AB, with a velocity AB. From the point A, draw at random any straight line, as AD. This line I will call the base line. Now, when the body moves from A to B, it will be found to have moved away from the base line AD, in the direction of the parallel lines, that is to say, in the direction of AP, by the distance CB. And we, therefore, say, in perfect truth, that the body moving in the line AB, actually moves, with reference to the base line AD, in the direction of AP with a velocity BC.

FIG. 2.



Now, again, imagine another set of parallel lines in the same plane, all parallel to AD, the base line of the direction AP; and let AP be the base line of the direction AD; let B be on the line BE in the direction of AD. Then, we might say, with perfect truth, that the body moving along the line AB actually moves, with reference to the base line AP, in the direction of AD, with a velocity FB.

But the figure ACBF is a parallelogram, and the line AB, the diagonal, represents in magnitude and direction the velocity of the body; and the sides CB and FB represent in magnitude and direction the velocities of the same body in the direction AP and AD, respectively. We, therefore, conclude that the velocity AB in the direction AB is equivalent to the velocity CB in the direction of AP, plus the velocity FB in the direction of AD; provided, each of these lines is taken as a base line for the direction of the other.

This reasoning proves that a body, moving in a straight line AB, may be truly conceived as moving in any two different directions, AP and AD, with different velocities, according to the geometric law of parallelograms.

Conversely, if a body should have to move in any two different directions, AP and AD, with the velocities of AF=CB and AC=FB, it will move on the diagonal AB with the velocity AB (the above provision understood).

Now, if you attribute to bodies the property of inertia, that is to say, conservation of velocity and of direction, it follows at once, that a body which has by any means imparted to it two different velocities in two different directions must move in the diagonal of a parallelogram, as enunciated by Newton; for, the law of inertia virtually asserts that the lines AP and AD are lines of directions: AP for the velocity AF, and AD for the velocity AC; and that they are at the same time base lines with reference to each other; AP being the base line for the velocity AC, which tends to move it away from the line AP, in which the body would otherwise move by inertia. The same is true of the line AD.

So that the law of parallelogram velocities follows immediately from the law of inertia—the 1st law of Newton; and it is absolutely independent of the law of forces—Newton's 2d law. The second law is only necessary to prove the composition of *forces,* not of *velocities.* Thus: because forces are, with the same body, proportional to the velocities, by the 2d law of Newton, therefore, what is true of the velocities must also be true of the forces.

But, even if we choose to dissent from the 2d law, the composition of *velocities,* imparted by forces in any way, remains unshaken, since it follows from the law of inertia.

The reasoning in this remark was suggested to me by Arts. 26, 27, and 255 of the *Natural Philosophy* by Kelvin and Tait.

Remark 3. It is not at all essential that the two impulsive forces, M and N, should act on the body simultaneously. For, even if the force M has acted on the body first, and the body has, therefore, been moving by inertia along the line AB, and then the force N acted upon it at the point A—the same result should follow, and the body should move along the diagonal AD. For, according to Newton, the action of a force, whether impulsive or constant, is the same whether the body acted upon be at rest or in motion. Kelvin and Tait, in their comment on the 2d law of Newton (Art. 253, *Natural Philosophy,* Part I), come to the following conclusion:—

"The same force will produce precisely the same change of motion in a body, whether the body be at rest or in motion with any velocity whatever."

In fact, Newton's reasoning can only be correct if understood to proceed on the assumption, rightly inferred by Lord Kelvin from Newton's 2d law, that a force (meaning a definite quantity of force) will produce the same change of velocity in a body, whether it be in motion or at rest; for, Newton's reasoning, in plain language, is this: while the body by the force M is carried from A to B, the force N, acting in lines parallel to AC (which constitute its direction), carries the body away from the line of (*previous*) motion AB by a distance AC, measured on lines parallel to AC; therefore, when the body ought to have been at the point B, the force N has succeeded in carrying it along the line BD parallel to AC, by the distance BD, equal to AC, the velocity imparted by the force N. From which mode of reasoning it becomes clear, that the force N must be just sufficient to give the body a velocity AC, even while the body is in motion with the velocity AB. But, if the assumption be not true, then, the force N, although sufficient to give the body a velocity AC while at rest, will not be sufficient to give the same body the same velocity AC when the body is in motion with the velocity AB.

More, if the assumption be not correct, then the proof of parallelogram forces is altogether inapplicable, when the forces act simultaneously; for, neither force M nor force N can produce the velocities AC and AB, respectively, in the body, when each has to contend with the motion produced by the other.

ART. 3. SUPPOSITIONAL PROBLEM.

Suppose we accept Newton's second law in its entirety; that is to say, that the measure of even a finite impulsive force is by momentum, and that, therefore, with the same body, impulsive forces are proportional to the velocities they produce. We will propose to ourselves the following problem:

The force M is represented by the line AB; the force N, by the line AC, equal and parallel to BD; the resultant force, by the line AD. This is so, because these lines represent the velocities produced by the various forces; and velocities are, by the second law, proportional to the forces producing them.

Now, these three lines AB, BD, and AD form a triangle ABD; and from geometry we know that the sum of the two sides, AB and BD, is greater than the third side, AD; that is to say, $AB+BD>AD$ (1).

But, AB represents the force M and BD—the force N, and AD the resultant force, which call R. Therefore,

$$M + N > R \qquad (2)$$

or

$$(M + N) - R = C \qquad (3)$$

C being any positive quantity.

Now, inequation (2) and equation (3) show, that in the composition of the forces M and N to form the resultant R, some part C of the forces M and N is not at all effective. The question now arises, what becomes of the part C which is ineffective, and why is it ineffective?

It will not do to say that the line AD is the *geometrical* sum of the lines AB and BD, as some would have us understand it, (See Ames's *Theory of Physics,* Art. 13), thus converting inequation (1) into a so-called geometrical equation. This will not do; for we are here concerned with actual concrete, physical forces—say different quantities of gun powder, or different quantities of electricity, etc. What becomes of the part of the force of the gun powder, or of the electricity, etc., which is not effective? You cannot make of definite quantities of electricity, or of the quantities of force in definite quantities of gun powder, any geometrical sum; you must sum them arithmetically.

Three different answers are possible to this question:—

(a) If this problem should have been proposed to the great Newton, his reply would, most assuredly, have been, that the part C of the forces M and N is absolutely lost. Why? Because the forces M and N, acting on the body under an angle BAC, are in some measure opposing each other's action; and the larger the angle the greater their opposition, and the smaller, therefore, the resultant. If the angle be zero, their opposition is also zero, and the resultant is then equal to the sum of the two forces M + N: the forces conspire. If the angle be 180 degrees, the largest angle possible, their opposition is also the largest possible, and the resultant, therefore, the smallest possible = M — N: the forces directly oppose each other's action. Therefore, if the angle BAC be intermediate between 0 degree and 180 degrees, the opposition of the forces will also be intermediate, and the magnitude of the resultant will also be intermediate between M + N and M — N; and the larger the angle, the nearer it is to 180 degrees, and, therefore, the nearer the magnitude of the resultant to M — N; (and conversely).

(b) Let us, now, propose the same problem to the modern physicist, who stoutly defends the law of conservation of energy, and who, nevertheless, accepts Newton's measure of a finite impulsive force by velocity (for the same mass), and who, then, unconsciously and through sheer inattention caused by habit, contradicts himself, as will become evident to himself when his attention is called to it. (See Art. 8, Sect. I, Book II.)

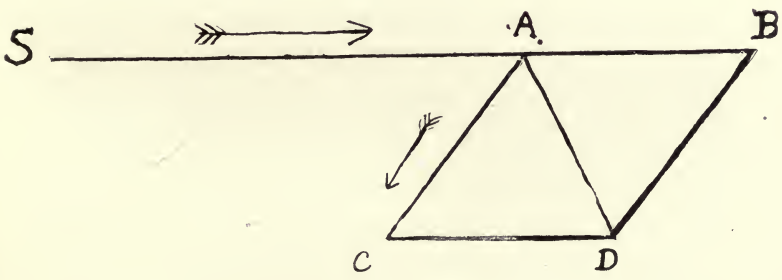
Let us propose it to him, and his first answer will probably be, "Why, the part C of the *energy* is *spent* in doing the *work* of deviating the resultant motion from both lines AB and AC; in a word, for changing the *direction* of motion. "Does not," he will say; "the law of inertia affirm that bodies *cannot of themselves* change the direction of motion, and in order, therefore, to effect a change of direction in a moving body, force is necessary. The body, in this case, has suffered a change of direction from either of the original directions communicated to it by the forces, and *energy* has, consequently, been *spent* in doing this work of changing direction. And, further, the larger the angle BAC, the greater the total deviation; and the greater the *work done,* the greater the amount of *energy spent,* and, therefore, the greater is C and the smaller is R."

(c) But, my dear reader, let us not let him off so easily; let us further press him and say to him, "True, the part C of the energy must have been, as you say, *spent* in performing the work of change of direction, but what has become of it? Has it been annihilated in performing the work? Do you not assert, that energy cannot be annihilated under any circumstances whatever? And, if so, what, then, has become of the part of energy C?"

If so pressed, we may with perfect confidence imagine him to reply thus: "True, my answer has been rather incomplete than wrong. The part C has really been spent in effecting the change of direction, as I said before; but while having been so *spent,* it, nevertheless, has not been annihilated, but it must have been, and I am most certain it really has been, converted into some form of molecular energy in the body acted upon: the particular form of the molecular energy generally being *heat;* for, wherever velocity disappears, without having been *converted* into *potential energy, heat* generally takes the place of the molar energy, so disappearing. Witness the case of the collision of two bodies moving with equal momenta in the same straight line but in opposite direction. All agree that in this case the motion lost is converted into the particular form of molecular energy, called *heat.*

"Let us illustrate our meaniny by a concrete example," we may imagine him to continue, "suppose a stone of unit mass to have been thrown by a force 'V' in any direction SA (Fig. 3). Ignore gravitation and

FIG. 3.



the resistance of the air, for the sake of simplicity. The velocity of the stone along the line SA will be equal to V, the mass being unity. Imagine a rope attached to the stone and moving along with it. Imagine, further, that at the point A the rope was suddenly pulled by some force V_1, in any direction AC, thus producing a velocity V_1 in the direction AC. Make AB in the direction of the force V equal to V. Make, again, AC in the direction of the force V_1 equal to V_1. Then the stone will move in the direction AD with a velocity equal to AD, which call V_2. Suppose V_1 to be very slightly smaller than V. Now, the angle between the directions of the forces is the angle BAC; and as we have taken this angle arbitrarily, we may imagine it to be very small or very large, that is to say, we may imagine it to be zero or 180 degrees.

"If the angle BAC be zero, that is to say, if the rope act in the line SA, and in the direction of the force V, no change of direction will take place, and, therefore, as no work will be done, no energy will be spent, and the effect of force V and V_1, on the body, will be purely molar; therefore, the velocity of the body, V_2, will be equal to $V+V_1$.

"But, if the angle be very large, that is, equal to 180 degrees, that is to say, if the rope act in the line SA, but in a direction contrary to the direction of the force V, then, the change of direction will be the largest possible, and, therefore, the amount of energy spent in effecting this change of direction (C) will also be the largest possible, and the molar effect of the forces on the body will be very small and equal to $V - V_1$, while nearly the whole of the forces $2V_1$ will have been spent in changing the direction of the body.

"But, in this last case, when the rope acts in a direction directly contrary to the motion of the body, and so stops its motion, or nearly so, all physicists of the present day agree that the entire motion $2V_1$, apparently lost, is, in reality, not lost, but is converted into heat.

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"Therefore, we infer, that all force *spent* in affecting change of direction is *converted* into *heat,* no matter what the change of deviation."

[illegible] physicists will be compelled to admit, in accordance with his own principles, that whenever and wherever composition of any forces takes place, *heat* is a necessary accompaniment, if the angle made by the components be more than 0 degree (and less than 180 degrees, if one of the components be gravitation, as we hope to explain further on).

ART. 4. THE REAL PROBLEM.

The problem in the above article is only a suppositional problem, because, there we supposed that the (kinetic) force of a moving body is proportional to its velocity; or, that the measure of a finite impulsive force is by momentum. But, in reality, this is not true; for we now know perfectly well that the (kinetic) energy (or force) of a body (of unit of mass), moving with any velocity, no matter how it has been imparted to it, whether by finite impulsive forces, or by what I call inexhaustible forces, that is, constant forces—is always and invariably proportional to the square of the velocity; and that, therefore, also the measure of a finite impulsive force is not by momentum, but by the measure of energy, that is by mass and half square of velocity. This is the perfect, positive truth, and there can be no question whatever about it. Thousands upon thousands of experiments in Thermo Dynamics as well as in every other department of physics testify to its truth, as we have repeatedly reiterated in many of the previous pages.

Now, if, as we did in the previous article, we begin to compare the sum of the energies of the components with that of the resultant in the composition of forces, a most embarrassing and absolutely insurmountable difficulty encounters us.

To explain, let us revert to the example of the stone with the rope attached, of our previous article. The stone was moving by inertia along the line SA with a velocity V. Its kinetic energy and the measure of the force spent in producing this velocity is equal to $\frac{V^2}{2}$

At the point A, a force imparted to the stone a velocity V_1 in the direction AC (Fig. 3). The kinetic energy represented by this velocity and the measure of the force spent in communicating this velocity (if the velocity actually communicated should have been more or less than V_1, the body would not move along the diagonal AD) is equal to $\frac{V_1^2}{2}$

The kinetic energy represented by the velocity V_2 is equal to $\frac{V_2^2}{2}$

Now, by the law of conservation of energy, it is required, that the sum of the energies of the components should be equal to the energy of the resultant; that is

to say, that the equation $V^2+V_1^2=V_2^2$ . . . (4)

or rather $V_2^2=V^2+V_1^2$ . . . (5)

should be true (provided, of course, that no energy should have been transformed into molecular or potential energy in the composition of the forces).

But the three velocities V, V_1, and V_2 are represented in the parallelogram (Fig. 3) by the sides of the triangle ABD: the side AB representing the velocity V; and side BD, the velocity V_1; and the side

AD, the velocity V_2; and, further, the angle ABD is equal to 180 degrees—angle BAC (angle BAC being the angle between the components).

From trigonometry we know that

$$AD^2 = AB^2 + BD^2 - 2AB.BD \cos ABD \quad . \quad . \quad (6)$$

Substitute the value of the angle ABD, as given above, into equation (6), and we get

$$AD^2 = AB^2 + BD^2 + 2AB.BD \cos BAC \quad . \quad . \quad (7)$$

Now, in equation (7) put V_2 instead of AD; V instead of AB; and V_1 instead of BD, and we finally get

$$V_2^2 = V^2 + V_1^2 + VV_1 \cos BAC \quad . \quad . \quad . \quad . \quad (8)$$

But the cosine of an angle may be either a positive or a negative quantity or zero. It is a positive quantity for all angles less than 90 degrees—acute angles; it is a negative quantity for all angles between 90 and 180 degrees—obtuse angles; and it is zero for angles of 90 degrees.

We therefore infer very easily from equation (8) that (a), if the angle BAC, the angle between the components, be an acute angle, then

$$V_2^2 > V^2 + V_1^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

contrary to the assertion expressed by equation (4); (b), if the angle BAC be an obtuse angle, then

$$V_2^2 < V_1^2 + V^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

contrary again to (4);

(c), only if the angle BAC is equal to a right angle is $V_2^2 = V^2 + V_1^2$, in accordance with (4).

In plain language, our meaning is as follows: By the law of conservation of energy, the total energy of the body moving along the diagonal AD should be neither more nor less than the sum of the energies spent for producing the two velocities V and V_1. In reality we find, firstly, that when the angle between the forces is an obtuse angle, the kinetic energy of the body moving with the resultant velocity V_2 is actually less than the sum of the energies spent by the two forces for producing the component velocities; secondly, that when the angle between the components is an acute angle, the kinetic energy of the resulting motion is actually *more* than the sum of the component energies.

As to the first case, when the angle in obtuse, the question is: What has become of that part of the energy of the components, which does not appear as molar energy in the resultant?

This may easily be answered by the third answer to our suppositional problem of the previous article; namely, that it has been spent for change of direction; and, while so spent, has been transformed into molecular energy—*heat.*

But as to the second case, when the angle is acute, and the energy of the resultant is *more* than the sum of the energies of the components, the question is: Wherefrom and how did arise that part of the energy of the resultant by which it is *more* than the total sum of the energies imparted to the body by the original forces; in a word, the body manifests more energy than was given to it by the forces. How is that possible? Does it not seem like a spontaneous creation of energy out of no energy? Is not this an absurdity?

The answer given above, in the first case, is, of course, entirely out of place here. On the contrary, that answer greatly enhances our difficulty. If energy has been spent for change of direction and converted into heat, the molar energy of the resultant ought to be *less,* than the energy of the components, but not *more.*

My dear reader, does it not seem to you as though nature was contradicting herself? On the one hand, nature incessantly testifies to the correctness of the law of the parallelogram of forces by the motion of the heavenly bodies, thus corroborating our reasoning from the very well-known properties of matter and motion (I allude to the law of inertia, which alone is sufficient to make the law of the parallelogram of forces a geometrical necessity.) Nature, again, teaches us that the measure of energy is such and such. Nature further affirms with great persistence that energy cannot be destroyed nor created (at least when only finite forces are concerned). (See Art. 27, Sect. II, Book I.)

All this nature seems to affirm with great positiveness. But, then, when we come to vertify her statements and compare the energy of the components with that of the resultant, measuring them by the measure she, herself, taught us to employ when dealing with energy, then, I say, we find to our great astonishment that nature, honest and true nature, has—shall I say it—deceived us and falsified herself; for her law of conservation is not borne out by these measurements.

But, my dear reader, nature, holy and true nature, never, never lies. We both may safely be surety for that. The fault, then, must be with us. We must have either misunderstood her statements or badly interpreted them, or, finally, badly measured either the components or the resultant. Let us, then, try to find wherein we are faulty; and to this purpose we shall, accordingly, devote our next section.

SECT. II. SOLUTION OF THE PROBLEM.

1. By way of preface, I desire to encourage the reader to persistence in this our enterprise of diligently searching for, and locating, the place of our error; thus exonerating nature and clearing her of many faults which have been imputed to her. For, I assure you dear reader, that it is my deepest conviction, that solely because physicists failed to put this little problem to themselves, many things which throw a shadow on the pure and bright character of nature, have been imputed to her. I cannot with positiveness assign any reason why physicists, the true and devoted servants of nature, have failed in this, their sacred duty, to propose this problem to themselves. Again and again have I, on this very account, said to myself, "No! It cannot be. You must be grossly mistaken, and there must be no such problem at all; for otherwise it would not have been passed in silence by the scientists." But, again and again have I, with assiduity and persistance, consulted and reconsulted all and every authority that I could get access to—only to come out of all this labor with a stronger and stronger conviction that I was right, and that there *is* such a problem.

It is rather astonishing and discouraging to notice, that those very great physicists, who, because they found in the phenomena of tides an apparent contradiction to the law of conservation of energy, grudged no labor to attempt a rectification of the difficulty; and although at first satisfied with the dogmatical assertion that the energy of the tides *must* be derived from the earth's rotation, have subsequently built on this foundation a magnificent structure of the finest and most attractive theories in physics, as "the determination of the earth's retardation," "the acceleration of the moon's revolution," "the enlargement of the moon's orbit," "the effect of tidal friction on the evolution of the cosmos," etc.—a structure, I say, in the raising of which an inestimable amount of costly mental labor has been spent, and the best mathematical, physical and astronomical skill employed—that these very physicists should have passed the more formidable apparent contradiction to convervation of energy, included in the "parallelogram of forces," in absolute silence, is, indeed, both astonishing and discouraging.

2. To our task, then. First of all let us arrange the elements of our problem. I mean all premises and all statements that are in any way related to it. They are as follows:—

Firstly, the law of inertia as understood by Newton; that is to say, the statement that a moving body tends as far as in it lies to conserve the velocity and the direction of its motion. The intensity of this tendency (inertia), as Newton would have it, being the same, whether the body be moving with a great or with a small velocity or not moving at all. The principle of parallelogram *velocities* rests solely on this law, as we have decisively proved above. (Remark 2, Parallelogram of Forces.)

Secondly, the statement quoted, in Remark (3) to parallelogram of forces, from the "Natural Philosophy" of Lord Kelvin and Tait, and reiterated by all physicists, as tacitly implied in Newton's laws, that "The same force will produce precisely the same change of motion in a body, whether the body be at rest or in motion with any velocity whatever."

This statement seems to me to be a necessary consequence of the law of inertia as generally understood. For, since the inertia of a body is the same whether it be at rest or in motion, there is no reason why a force should *not* produce in a body the same change of motion, whether the body be at rest or in motion with any velocity whatever.

Thirdly, conservation of energy.

Fourthly, the measure of energy by mass and half the square of velocity.

Fifthly, The law of parallelogram velocities.

These, as far as it seems to me, are all the principles involved in our real problem.

3. We will now begin to examine closely every one of these statements, in order to find which one of these is incorrect. We have proved above that there must be something wrong about the law of parallelogram of forces; and as these are all the principles involved in the "parallelogram of forces," the *something wrong* must be included in one or more of these five statements.

4. We will begin with the last, and say, that the law of *parallelogram velocities* (not forces), is, without the slightest shadow of a doubt, correct. By the law of parallelogram velocities I mean something like the folowing statement:—

If two velocities are somehow (no matter what the expenditure of energy might be to produce these two velocities) imparted to one and the same body (whether simultaneously or in succession) in two different directions, then, will the body move in a direction and with a velocity represented by the diagonal of a parallelogram, whose two contiguous sides represent in magnitude and direction the velocities imparted to the body.

In this law, as stated here, we omit all reference to the measure of force in general, or to the measure of the quantities of force expended for producing these component velocities; for, the proof of the law of parallelogram velocities is absolutely independent of these two measures; for, it follows directly from the law of inertia, as we have conclusively proven in our second remark to the parallelogram of forces. Granted that any two quantities of energy (we don't care to know what the real quantities are) will, and are sufficient to, produce the velocities AB and AC, respectively, and it immediately follows from the law of inertia, that the body thus acted upon must move with the velocity AD and in the direction AD.

So, then, our fault does not lie in the law of parallelogram of forces, if by forces you mean velocities; and this substitution of *velocities* in place of forces does ont help us to rectify our difficulty.

5. Our fault cannot possibly be included in our fourth statement, for it rests solely on experiments. The same is true of our third statement, as far as finite froces are concerned.

6. We now come to the second statement; namely, that a force, whether finite or infinite, will produce the same change of velocity in a body, whether the body be at rest or in motion with any velocity. This important assertion has never, as far as I know, been proved experimentally or otherwise in the case of a finite force. It has merely been inferred from the language employed by Newton in the expression of his second law; and, because of the authority of Newton.

has never been questioned. In the case of the only infinite force in nature, *gravitation,* experience, and experiments, indeed, teach us that the case is undoubtedly so: that a gravitation of a certain intensity will produce in a body, always and invariably, the same change of velocity in a definite interval of time, no matter whether the body be at rest or in motion with any velocity whatever. But, in the first place, gravitation, being an infinite and inexhaustible force, is widely different from all other forces, which are but finite and limited; and we, therefore, have no right to infer from gravitation that the case is also such with other forces. You might as well infer from gravitation that the same finite force will produce the same change of velocity in any body, no matter what the mass, for such is the case with gravitation. (See Art. 10, Sect. I, Book I. See, also, School. 2, Prop. Sect. III, Book II.)

In the second place, when we say of gravitation that the *same force* produces the same change of velocity, no matter whether the body be at rest or in motion, we mean by "the same force," the same *intensity,* not the same *quantity* of force; for, even with gravitation a greater quantity of force is exerted for the production of a given change of velocity when a body is in motion than when it is at rest. Let the given change of velocity be "V_1," then, if the change of velocity be from rest, the energy acquired by the body, and, consequently, the quantity of force exerted by gravitation, will be equal to $\frac{V_1^2}{2}$ (the mass being unity); but if the change of velocity be from motion, with a velocity "V," then will the amount of increase (or diminution) of energy be equal to

$$\frac{(V+V_1)^2}{2} - \frac{V^2}{2} = \frac{V^2 + 2VV_1}{2}$$

$$\text{but } \frac{V_1^2 + 2V.V_1}{2} > \frac{V_1^2}{2}$$

therefore, the assertion that a force (meaning, as generally understood, a definite quantity of force) will produce the same change of velocity in a body, whether it be in motion or at rest, not only lacks all proof (barring Newton's authority), but is, in fact, contrary to the laws of energy.

7. Let us now turn to the first statement, that of inertia; and we say, that if the intensity of inertia be he same whether a body be at rest or in motion with any velocity, then, must the second statement be true; for, on this supposition, there is no reason why more energy should be required to produce a change of velocity in a body in motion than when it is at rest; and, if our second statement be proven to be false, then must part of our first statement, namely, that "the intensity of inertia is invariable," also be false.

8. What will be the bearing of these reasonings on the problem at hand? It is this: If inertia varies with and is proportional to the velocity of the moving body (See Prop. 8, Sect. III, Book II), then, of course, a greater amount of energy wil be required to produce a certain change of velocity in a moving body than in one at rest; and, therefore, is the energy of the component "V_1" in the example of the stone with

the rope attached (Art. 3, Sect. I, Book III) *more* than $\frac{V_1^2}{2}$, and, therefore, is the sum of the energies of the two components more than $\frac{V^2+V_1^2}{2}$; and this will entirely remove our difficulty, when the angle is acute and the component forces act in succession.

We have thus found that our fault *must necessarily* lie in a misunderstanding of the law of inertia; namely, in our assuming that inertia does not vary with the velocity of the body. This circumstance will afford strong corroborative proof of Proposition 8 of Sect. III, Book II. For, if that proposition be rejected, then, energies $\frac{AB^2}{2}$ and $\frac{AC^2}{2}$ ought to be *sufficient* to produce the velocities AB and AC in one and the same body, no matter what the angel of direction between the two energies; and, if *sufficient* to produce the velocities AB and AC, the body *must* (as we infer directly from the law of inertia in our second remark to parallelogram force) necessarily move in the direction AD, with the velocity AD; and that means, that $\frac{AD^2}{2}$ must be equal to or less than $\frac{AB^2}{2}+\frac{AC^2}{2}$ while, in reality, when the angle is acute, $\frac{AD^2}{2}$ is more than

$$\frac{AB^2}{2}+\frac{AC^2}{2}$$

9. A result directly following from our acceptance of Proposition 8 of Sect. III, Book II, and the consequent rejection of the statement that *any* force will produce the same change of velocity in a body, whether it be at rest or in motion with any velocity whatever;

a result following directly from this is, that Newton's mode of reasoning in proving the law of parallelogram *forces* becomes valid only when both forces concerned or at least one of them (namely, that producing the second component) is gravitation [which, as we know *will* really produce the same change of velocity in a body, whether it moves with this or with that velocity. (See Scholium 2, Prop. 8, Sect. III, Book II)]; for, if both forces be finite, then, Newton's mode of reasoning is entirely inadequate, as we have pointed out in our third remark to the parallelogram of forces in section I of this Book. But, it is only inadequate, if by *forces* we really mean the quantity of the forces concerned; but, substitute *velocities* in place of *forces*, and Newton's reasoning, as well as his conclusion, is perfectly correct.

BOOK IV.

PARALLELOGRAM
OF
ENERGIES.

BOOK IV. PARALLELOGRAM OF ENERGIES.

In the two previous sections we have conclusively shown, firstly, that the old Newtonian conception of the parallelogram of forces is entirely erroneous, except when the two forces concerned are gravitations; secondly, that its principal defect lies in the assumption that inertia is invariable and constant, and that, therefore, a definite quantity of force will always and invariably produce the same change of velocity in a unit of mass; thirdly, that, nevertheless, if two definite quantities of force or energy, E and E_1, act in different directions upon one and the same body; and, if both energies, E and E_1, be sufficient to produce the velocities V and V_1, respectively (each in its own direction), even while acting conjointly in case of simultaneous action; or, in case of action in succession, if the energy E_1 be sufficient to produce in the body a velocity V_1 in its own direction, even while the body is moving with velocity V, which has been previously produced in it by the energy E——————, that under such conditions we may conclude apriorily from the law of inertia (and in case one of the forces is gravitation, we see it vertified in nature in the motion of the heavenly bodies) that the body so acted upon must move with a velocity V_2, represented in magnitude and direction by the diagonal of a parallelogram, whose contiguous sides represent in magnitude and direction the velocities V and V_1.

Now, these conclusions once accepted, as they necessarily must be, and it follows, that in the present, there is no known law for composition of forces at all; I mean, that at present there is no law in the science of physics by which to determine the relations of the quantities of force or energy expended for producing components to that of the resultant; and we are entirely at a loss to know:—

(a) What amount of energy is necessary to produce the second component V_1 in a body moving by inertia with a velocity V?

(b) What is the law of motion when two given quantities of energy act on a body simultaneously in different directions?

(c) What part of the energies of the components does not appear as molar motion in the resultant; and what is the relation of this part to the component eneriges?

(d) Whether or not, if the force producing the second component be gravitation, any energy at all is lost as far as molar motion is concerned, and must, therefore, appear in the body acted upon as molecular motion; as is positively the case when the force producing the second component is finite?

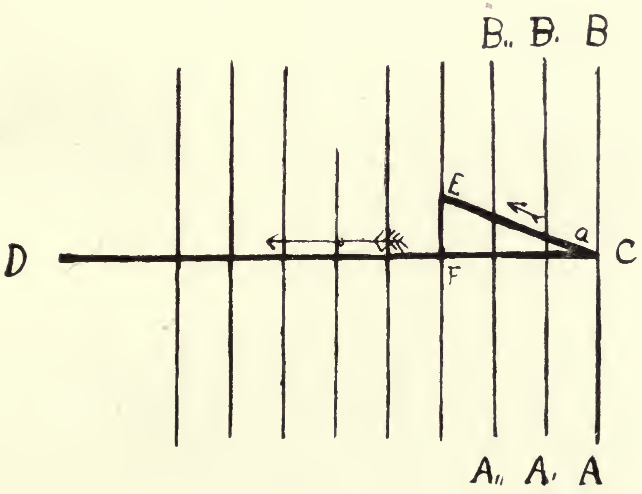
In this book we intend to consider these questions along with many others, relating to the composition of motive energies; and this is why we term this book *Parallelogram of Energies.*

We will, for the sake of system, arrange what we have to say in this respect in the form of lemmas, propositions, etc., as we have done on a previous occasion.

Lemma 1. If two quantities of finite forces (or eneriges) act on a body at rest, whether simultaneously or in succession, in the same direction, then, the energies conspire, and the kinetic energy produced fully represents the sum of the two energies. But, if the two forces do not act in the same direction, then, the forces, in some measure, oppose each other, as far as molar motion is concerned; and the energy of the motion produced is less than the sum of the energies producing the motion. The degree of their opposition depends on the angle between the forces, being less with a smaller angle and more with a larger angle; so that when the angle is 180 degrees, the largest possible angle, they fully oppose one anohter, as far as molar motion is concerned; and the energy of motion produced is, then, equal to the difference of the energies.

Lemma 2. Let the parallel lines (Fig. 4) AB, A_1B_1, $A_{11}B_{11}$ . . . constitute any direction. Draw the indefinite straight line CD perpendicular to the parallel lines. Then, we say, that a body moving in the direction of CD does not move at all in the direction of AB; for, a body may move along the line AB either from A to B, or from B to A, the two motions being

.FIG 4



opposite one to the other, and since the line CD is inclined neither to A nor to B, therefore, a body moving along the line CD, moves neither from A to B nor from B to A; that is to say, it does not move in the direction of AB altogether.

Further, if a body be moving along the line CE, which makes an angle a with the line AB, with a velocity CE, from C to E, then, as evident from Fig. 4, the body will be moving in the direction AB from A to B, with a velocity EF; that is to say, with a velocity CE cos. a.

PROPOSITION 1. If a body be moving along the line CE (see Fig. 4) with a velocity CE, from C to E; or, if a force, finite or infinite, tend to produce a motion in the body along the line CE, from C to E, with a velocity CE, in the case of a finite force, or, an intensity CE in case of an infinite force; and, if the body encounter a resistance to its motion, and the direction of the resistance to motion (from A to B) be in the lines AB, A_1B_1,$A_{11}B_{11}$ . . . from A to B; then, in case of motion or a finite force, will the body move in the line CD, with the velocity CF = CEsna; and, in case of an infinite force of intensity CE in the line CE, from C to E, the force will act in the line CD, with an intensity CF, equal to CEsna.

Demonstration. Firstly, Direction.

The direction of the motion or of the force must be along the line CD, because the line CD is the (first) line where the resistance to motion is zero, by lemma 2; since by supposition, the direction of the resistance is in the direction AB.

Secondly, (a), magnitude of motion, in case of finite force or motion; (b) magnitude of intensity in case of infinite force.

(a). Magnitude of Motion. (Finite Force.)

The energy of the force or motion is equal to $\frac{CE^2}{2}$ by supposition. The resistance destroys (as far as molar motion is concerned), and can only possibly destroy, that part of the motion which is in its direction, which, by lemma 2, is equal to EF, which is equal to $CECosa$; the energy of the part destroyed is, therefore, equal to $\frac{CE^2\cos^2a}{2}$; the balance of the energy by which the body moves along the line CD, is, therefore, equal to

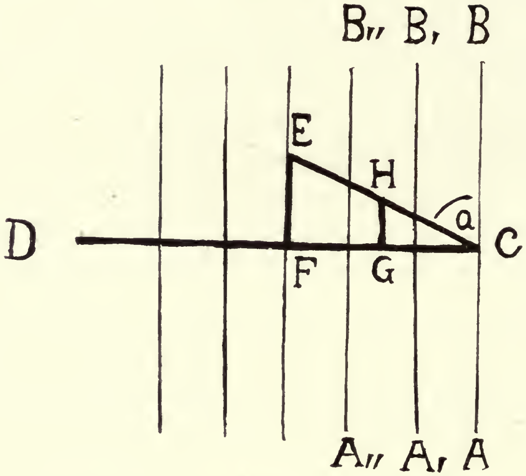
$$\frac{CE^2—CE^2\cos^2a}{2}=\frac{CE^2(1—\cos^2a)}{2}=\frac{CE^2Sn^2a}{2}=\frac{CF^2}{2}$$

But an amount of energy equal to $\frac{CF^2}{2}$ will communicate to a body at rest a velocity equal to $CF=CE\ sn(a)$, which proves our proposition as far as finite forces are concerned.

(b). The Magnitude of Intensity in Case of Infinite Force.

The given intensity is CE; therefore, its microt is equal to $\frac{M}{CE}$, where M represents the microt of a force of unit intensity (see Prop. 7, Sect. III, Book II). In Fig. 5, let CH be the magnitude of the first microf, which take as unity of velocity; the energy of the microf will, then, be represented by $\frac{1^2}{2}$.

FIG. 5



But the resistance destroys an amount of energy

$$\frac{HG^2}{2}=\frac{\cos^2 a}{2}$$

the balance of energy of the microf will, therefore, be equal to

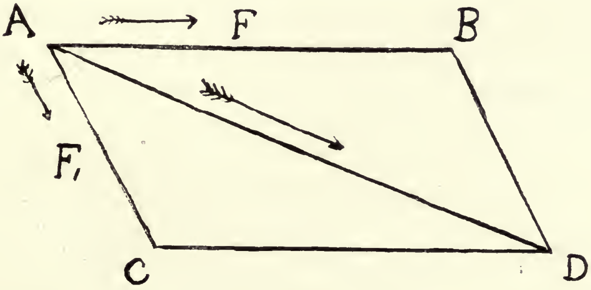
$$\frac{1-\cos^2 a}{2}=\frac{Sn^2 a}{2}=\frac{CG^2}{2}$$

and this energy will only suffice to produce a part of a microf equal to sna, during the microt $\frac{M}{CE}$; and, in order for the force to produce a full microf, which is the least possible quantity of motion, the time for its production must be equal to $\frac{M}{CEsna}$; and this will, therefore, be the magnitude of a microt of efficient force along the line CD; and as the intensity of forces is inversely proportional to their microts, therefore, will the intensity of the efficient force be equal to CEsna. Q. E. D.

Scholium. In nature we find this proposition verified to perfection, as examplified, for motion or finite force, by the resulting motion of a body in motion striking a resisting surface at an angle; and, for the intensity of an infinite force, by the well-known laws of gravitation, acting on a body supported on an inclined plane. The direction of the resistance in these cases being along lines perpendicular to the surface.

PROPOSITION 2. If two *infinite forces* of intensities F and F_1, respectively, act simultaneously on a body at rest, in different directions, the direction of the forces making any angle (a) between themselves, then, will the efficient intensity and direction of the combined two forces be represented by the diagonal of a parallelogram, whose contiguous two sides represent, in magnitude of intensity and in direction, the two forces F and F_1.

FIG. 6

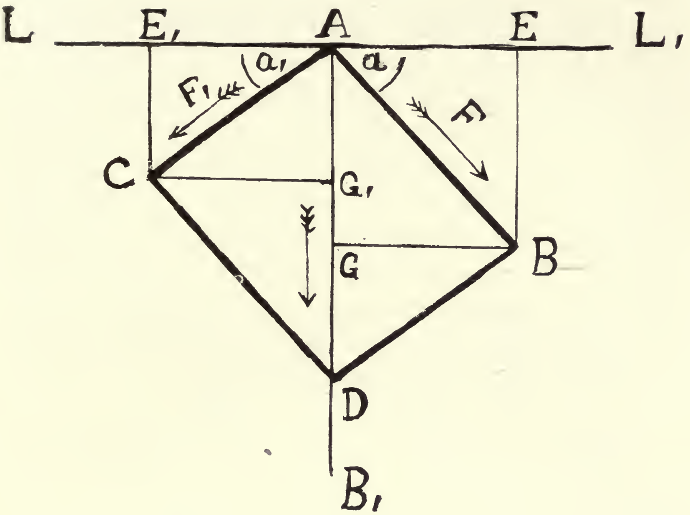


In Fig. 6, let A be the point of application of the two forces F and F_1, and AB and AC represent the magnitude of intensity and the direction of these two forces. Complete the parallelogram ABDC; and we say, that the diagonal AD will represent in magnitude of intensity and in direction, the efficient intensity and direction of the two forces F and F_1 combined.

Demonstration. Since the two forces do not conspire, they, therefore, to some degree, oppose each other's action (lemma 1); that is to say, they offer resistance one to another. Therefore, by Proposition 1, the motion resulting from the force F will be along a line perpendicular to the direction of the resistance offered to it by the force F_1; similarly, the motion resulting from the force F_1 will be along a line perpendicular to the direction of the resistance offered to it by the force F. Now, as a body can possibly move (simultaneously) only in *one* line, therefore, this *one* line must simultaneously be perpendicular to the direction of both resistances; from what we infer, that both resistances are in direction along one and the same straight line, perpendicular to the line of the resulting motion.

Let, now, the indefinite line AB_1, in Fig. 7 be the direction of the resulting motion; the indefinite line LL_1 perpendicular to the line AB_1 will, then, represent the line of direction common to both resistances. Drop the perpendiculars BE and CE_1, and also the perpendiculars BG and CG_1. The line AE is equal to F cosa. But, the angle (a) is the angle which the force F makes with the line of the resistance LL_1, offered to it by the force F_1; therefore, $AE = F\cos.(a)$ represents, by Lemma 1, the part of the intensity of the force F destroyed by the resistance offered to it by the force F_1. In a similar manner it may be proved that $AE_1 = F_1 \cos a_1$ represents the part of the

FIG. 7



intensity of the force F_1 destroyed by the resistance offered to it by the force F. But, the resistance offered by the force F must be equal and opposite to the resistance offered by the force F_1; for, if not, motion would result along the line LL_1*; therefore, $AE = AE_1$ . . . (1).

Now, AG is equal to Fsn(a); therefore, by Prop. 1, it represents the efficient intensity of the force F, along the line AB_1. In a similar manner, AG_1, the equal of $F_1 sn a_1$, represents the efficient intensity of th force F_1 along the same line. Therefore, the whole intensity of the two forces combined along the line of motion AB_1 will be equal to $AG_1 + AG$.

Now, make GD equal AG_1, and the whole intensity along the line AB_1 will be represented in magnitude (as well, of course, as in direction) by the line AD. Connect D with B and C, and it may be easily proved that the figure ABDC is a parallelogram; for, BG is equal to AE, and is, therefore, equal to AE_1 by equation (1); and GD is equal to AG_1 by construction; therefore, is the right angled triangle BGD equal to the right angled triangle AE_1C; for, $CE_1=AG_1$ and, therefore, is BD equal to AC. BD is also parallel to AC; for, the angle GBD is equal to the angle E_1 AC, and, as the line GB is parallel to the line E_1A, therefore must the line BD be parallel to the line AC. Therefore is the figure ABDC a parallelogram; and as AD, the diagonal of this parallelogram, has been proved to represent in magnitude and direction the whole intensity of the two forces combined, therefore is our proposition completely proved.

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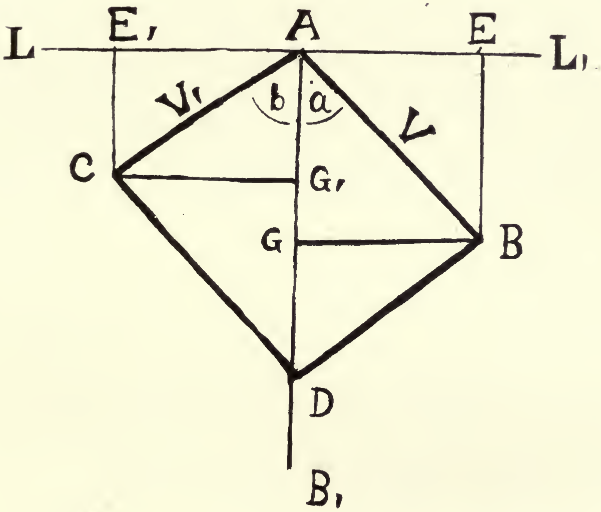
*For these are not static but dynamic resistances.

Scholium. Since in statical mechanics we mean by a "force" the intensity of the force, not its absolute quantity, that is, the quantity of its energy—for, statics is the science of equilibrium, not of motion—therefore, is this proposition, about the composition of intensities, the basis of all statical mechanics, no matter whether the forces concerned in equilibration be finite or infinite. But, outside of statics, in dynamics, this proposition is true of *infinite forces only;* that is to say, of gravitational forces only, as will be seen from the next proposition.

PROPOSITION 3. If two *finite impulsive forces* act simultaneously on a body in two different directions, then, will the direction of the resulting motion be represented by the diagonal of a parallelogram, whose contiguous two sides represent in direction the direction of each force, respectively, and in magnitude the velocity which each force is capable of communicating to the body, if acting alone, from a state of rest.

Further, the velocity of the resulting motion will be equal to the square root of the sum of the squares of the two products of the velocity of each force, by the cosine of the angle which the direction of each force makes with the diagonal of the parallelogram.

FIG. 8



In Figure 8, let AB and AC represent the velocities which each force is capable separately to communicate to the body from a state of rest, which call V and V_1.

Complete the parallelogram ABDC, and we say that the direction of the resulting motion will be along the diagonal AD; and, further, that the velocity of the resulting motion will be equal to $\sqrt{AG^2+AG_1^2}$ AG being equal to $V \cos a$ and AG_1 equal to $V_1 \cos b$.

Demonstration. As in the preceding proposition it may be proved that the line of motion, AD, must be perpendicular to the common line of resistance, LL_1; also, that line AE, by proposition 1, represents the amount of velocity of the energy $\frac{V^2}{2}$ destroyed by the resistance offered to it by the motive energy $\frac{V_1^2}{2}$; also, that AE equals AE_1, and that, therefore, AD is the diagonal of the parallelogram ABDC, as proved above. Also, by proposition 1, that AG ($= V \cos a$) represents the velocity by which the body moves along AD by the motive energy $\frac{V^2}{2}$, and that AG_1 ($= V_1 \cos b$) represents the same velocity by the motive energy $\frac{V_1^2}{2}$; therefore, the energy along the line AD equals to $\frac{AG^2+AG_1^2}{2}$, and the resulting velocity must be equal to $\sqrt{AG^2 + AG_1^2}$.

Q. E. D.

Scholium. The difference between the lines AE and AE_1 of this proposition and the similar lines AE and AE_1 of the preceding proposition, should be noted. In the preceding proposition lines AE and AE_1 denote intensities merely, and, therefore, have no energy values; while, in this proposition, AE and AE_1, denote actual finite energies, their energy value being

$$\frac{(\mathrm{Vsna})^2}{2}=\frac{(\mathrm{V_1snb})^2}{2}$$

And, although, as motive energies, AE and AE_1 antagonize each other, since they are directed to contrary parts; still, since they are actual finite energies, they cannot be annihilated; and, therefore, since they do not appear as molar energy, they must appear as molecular energy—heat.

We, therefore, unhesitatingly conclude that in every composition of two finite forces acting simultaneously on a body at rest, *heat* is a necessary accompaniment of the resulting motion (except with an angle between the forces equal to zero). Further, that the amount of heat thus developed, depends on the magnitude of the angle between the forces, attaining its maximum with a maximum angle of 180 degrees.

Remark. A whole volley of criticism is likely to follow upon this, our third proposition, since, by it, the velocity of the resulting motion is not represented by the diagonal AD, as is the accepted opinion of today. But, I hope, that the candid and sober reader will see for himself, that from the data of the well-known laws of motion and of energy, this, our third proposition, is a necessary conclusion; and, further, that the Newtonian reasoning, upon which is based the proof of the parallelogram of forces, has no application in this case, as has been noted above.

PROPOSITION IV. There are two kinds of inertia: Firstly, the inertia of the quantity of motion, that is to say, the inertia of velocity; secondly, the inertia of direction. In other words, a moving body offers resistance, firstly, to a change of its velocity, and, secondly, to a change of its direction, even when no change of velocity is involved with it.

Demonstration. That there is such a thing as the inertia of direction, distinct and apart from the inertia of motion, is, as positively clear as that there is inertia of motion. Deny this inertia of direction, and the existence of what you call centripetal forces for causing circular motion becomes unnecessary; since in circular motion the velocity is uniform, and there is, consequently, no change of motion at all. The only change there is, is a change of direction; and, if a moving body offer no resistance to a change of direction, no force should be necessary to produce that change of direction; more, no force would be capable of producing change of direction, pure and simple; since, on no resistance no force can act. (See Proposition 1, Sect. III., Book II.)

Scholium 1. In fact, what you call by that mysterious name "centrifugal force" is nothing else but the resistance of the body to a change of direction.

Newton, in attempting to explain his meaning of a centripetal force, in definition V, was obliged, first, to postulate a centrifugal force, or what he calls "an endeavor to recede from the axis of motion."

"A stone, whirled about in a sling, endeavors to recede from the hand that turns it; and, by that endeavor, distends the sling and that with so much greater force, as it is revolved with greater velocity, and as soon as ever it is let go, flies away. That force, which opposes itself to this endeavor * * * I call centripetal force." (Principia, Def. V.)

Why, I ask, should a body endeavor to recede from the axis of motion? And what is this endeavor, if not the resistance to a change of direction? As a matter of fact, this kind of inertia is tacitly implied in Newton's definition of inertia; for, Newton defines inertia as the power by which every body "endeavors to preserve in its present state, whether of rest or of moving uniformly *forward in a right line:* "moving uniformly" really refers to the inertia of motion, "*forward in a right line*"—inertia of direction.

But Newton failed consciously and distinctly to distinguish between the two kinds of inertia; and, therefore, when he comes to speak of centripetal force he must need create a new and mysterious endeavor of rcession from the axis of motion, never referring this endeavor to inertia (at least distinctly so).

Scholium 2. The inertia of direction is a property of motion only, and not of rest; for, a body at rest, although resisting motion, cannot be said to resist a change of direction, since rest has no direction whatever. And, therefore, is this force of inertia of direction the only distinguishing mark of real motion, as contradistinguished from relative motion.

"The effects which distinguish absolute from relative motion, are the forces of receding from the axis of circular motion." (Principia, Scholium to Def.).

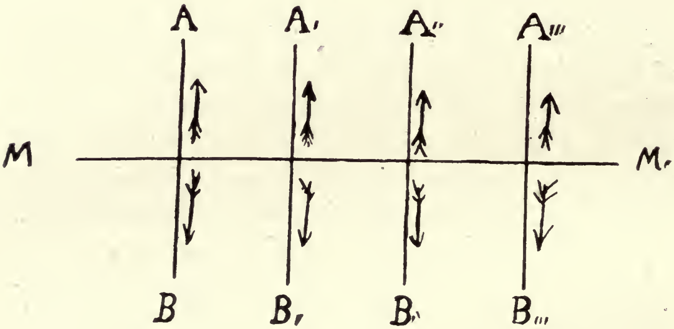
Scholium 3. Because forces overcome resistance (Prop. I, Sect. III, Book II), and because resistances are (by *this* proposition) of two kinds—resistance of motion and resistance of direction—therefore, forces are also said to be of two kinds: *motive forces* purely, when the forces cause motion in the right line of motion only, and *centripetal forces* purely, when the forces cause change of direction purely, as is the case in circular motion. With other culvilinear motion, elliptic, parabolic, and hyperbolic, the causative forces effect both changes simultaneously—change of motion and change of direction.

It should be remarked, however, that even change of direction can only be produced by *motion*. (See Art. 4, Sect. I, Book I.)

PROPOSITION 5. The direction of the resistance of direction (directional inertia) is perpendicularly across the line of motion, and is, on both sides of the line, directed away from it.

Let MM_1, in Figure 9, be the line of motion, and

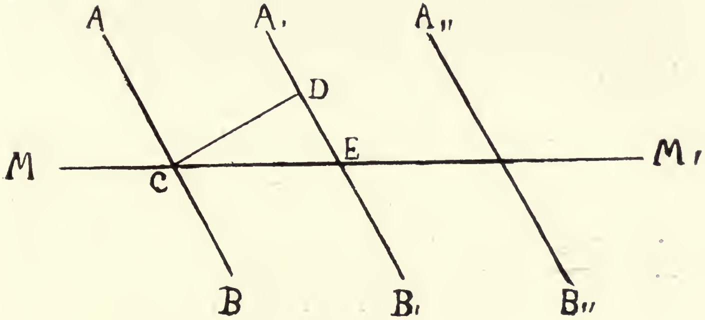
FIG. 9.



we say, that the direction of the directional resistance is along the parallel lines AB, A_1B_1, A_2B_2, etc., perpendicular to MM_1; and, further, that on both sides of line MM_1 it is directed away from MM_1, as indicated by the arrows.

Demonstration. (a). On apriori ground, the directional resistance cannot be directed in any direction oblique to MM_1; for, in that case, the motion of the body would be against a resistance, by Lemma 2, as shown in Figure 10. For, let the body have moved in

FIG 10.



any unit of time from C to E, then, by Lemma 2, it will have moved in the same time from D to E, in direction of the resistance AB, A_1B_1, A_2B_2. (The line CD is supposed to be perpendicular to the parallels.)

And to say that a freely moving body is actually moving against a resistance is the acme of absurdity.

Another simple consideration will also make it clear. Since the resistance of direction is really a resistance to dislodgement of the body away from the line MM_1, therefore, it must be directed along perpendicular lines which are directly away from the line of motion; for other inclined lines are, by Lemma 2, in part directed in the line of motion MM_1.

From this reasoning it is also obvious that the resistance on both sides must be directed away from the line of motion.

(b) Aposteriorily. Experience proves that the centrifugal force, which is nothing else but the resistance of direction, is, in direction, perpendicular to the line of motion. It is also directed away from the centre, because the resistance is, on that side, toward the centre and away from the line of motion.

Q. E. D.

PROPOSITION 6. Directional resistance varies with, and is proportional to, the velocity of the moving body.

Demonstration. We have proved in Sect. III, Book II, that the inertia of motion varies with and is proportional to the velocity of the moving body; and since it is one and the same body that offers both kinds of resistances, simultaneously, therefore, we must conclude that the inertia of direction, like the inertia of motion, must, also, be proportional to the velocity of the body. This is apriorily. Aposteriorily, we find from experience that the centrifugal force in circular motion, which is, as we have said, nothing else but a manifestation of directional inertia, varies with and is proportional to the velocity of the body. Thus, Newton, in the passage quoted above in Schol. 1, to Prop. 3, says: "and that with greater force as it is revolved with greater velocity"; and, in his scholium to the definitions, he, again, says: "but in true and absolute motions they (the forces of receding from the axis of circulation) are greater or less, according to the quantity of motion."

Scholium 1. If Newton should have rightly conceived the centrifugal force as a manifestation of inertia, he would not have asserted that inertia is invariable and constant; and, there would have been no reason for the writing of this volume; for, once admit that inertia is proportional to the velocity and all inferences and conclusions drawn and to be drawn in this present volume, follow as a necessary consequence.

Scholium 2. The formula for a centripetal force of circular motion, $f = \frac{V^2}{r}$, first discovered, I believe, by Huygens, may be easily inferred from this proposition. Thus: since the curvature of a circle is inversely proportional to the radius, therefore, the inertia of direction overcome, when a body moves in a circle, must be inversely proportional to the radius of the circle; and, therefore, the force overcoming the inertia must also be inversely proportional to the radius of the circle, or $f \propto \frac{1}{r}$

Further, as the inertia of direction varies with, and is proportional to, the velocity, and as any given arc of any circle is described in times inversely proportional to the velocity of the body—therefore, if one force can deviate a body of a velocity V, and can compel it to move in a circle of given radius, and, if another force can deviate the same body and can compel it to move in the same circle when it moves with a velocity V_1, the two forces must be directly proportional to the square of the velocities.

For, let the amount of resistance to be overcome for a definite arc of the circle be denoted by R when the velocity is V; then, for the velocity V_1, the resistance for the same arc will be $\frac{RV_1}{V}$. Now, since the intensity of a force is inversely proportional to the time it requires for overcoming any resistance, therefore, the amount of resistance overcome, divided by the force, will give the time required for overcoming the resistance.

Therefore, denoting the intensity of the centripetal forces for V and V_1 by f and f_1; and the times required for overcoming the resistances of any given arc by t and t_1, we get:—

$$\frac{R}{f}=t \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2)$$

$$\frac{R\times\frac{V_1'}{V}}{f_1}=t_1 \quad . \; . \; . \; . \; . \; . \quad (3)$$

Divide equation 2 by 3 and we get

$$\frac{V_1 f}{V f_1}=\frac{t_1}{t} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (4)$$

but $\frac{t}{t_1}$ is equal to $\frac{V_1}{V}$; therefore

$$\frac{V_1 f}{V f_1}=\frac{V}{V_1} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (5)$$

or $$f_1 : f = V_1^2 : V^2 \quad . \; . \; . \; . \; . \; . \quad (6)$$

or $$f \infty V^2 \quad . \; . \; . \; . \; . \; . \; . \quad (7)$$

Combine the two proportionalities (1) and (7) and we get

$$f \infty \frac{V^2}{r} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (8)$$

or, converting the proportionality into an equation

$$f=\frac{V^2}{r} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (9)$$

PROPOSITION 7. Energy (or force) must be spent for effecting a change of direction in a freely moving body, just as energy (or force) must be spent for a change of velocity in the moving body.

Demonstration. This is evident; since there is, by the preceding proposition, a resistance to change of direction, and since to overcome any resistance force or energy must be spent. (Prop. I, Sect. III, Book II.)

Scholium. It may be asked: If force must be spent for change of direction, how is it then that a body attached by a rope to a fixed centre, if set revolving around that centre, and if no other forces interfere, will continue to revolve around that centre for ever and ever with the same velocity, although there is a continual change of direction; where is the force that must be spent for this continual change of direction (as you say) derived from? Similarly, in a body rotating around an axis, as the earth, for instance, all particles of matter composing that body suffer continual change of direction. Wherefrom, therefore, is derived that force that must be spent for change of direction? Positively not from the energy of rotation; for, the angular velocity of rotation will go on forever and ever undiminished.

To answer this question we must remind the reader of what we said in Sect. III, Book II, that there are in the universe only two kinds of resistance: the resistance of inertia (including both kinds of inertia), and cohesive resistance. In the case of a body attached by a rope to a fixed centre, the directional inertia on the central side of the body is directed toward the centre (Prop. 5); the cohesive resistance of the rope is directed away from the centre. The two resistances, therefore, are directed in opposite directions, and, therefore, counteract each other. And as the cohesive resistance is, by supposition, the greater of the two, the inertial resistance on the central side of the body is over-balanced by the cohesive resistance; and, therefore, is the body on its central side, guided in its motion entirely by this cohesive resistance, and describes a circle without there being any necessity for force to be spent for overcoming the inertial resistance, since the inertial resistance has been counterbalanced by the cohesive resistance. Similarly with rotation. In the case of a loose body resting on the earth by gravitation and not by cohesion, the gravitational force in this case must be looked upon as a resistance and not as a force: not as a force, because it cannot produce motion toward the earth's centre on account of the cohesive resistance of the earth's substance. The body, therefore, describes a circle with its velocity undiminished (because of the inertia of motion); but when a body is moving freely its inertial resistance of direction is not counterbalanced, and force or energy must be spent for any change of direction.

The circle described by a body whose inertial resistance of direction has been counterbalanced by another resistance (cohesive), I propose to call the *Static Circle,* in contradistinction to the circle described by a freely moving body through the action of a force (centripetal force), which I propose to call the *Dynamic Circle.*

The difference between the two, from a physical standpoint, is vast and enormous, as will be gathered from what is to follow.

There is another difference between the static and dynamic circle, which we will note right here. The difference is geometrical. The static is an essentially geometrical circle; I mean it is a *continuous* curved line (if that be conceivable). The dynamic circle, on the other hand, must be conceived as a symmetrical polygon, inscribable in, or circumscribable by, a circle, but not as a continuous curved line; the same must be true of the dynamic (centripetal) ellipse, parabola, and hyperbola. All these curves, if produced dynamically, are composed of *real straight lines* at various angles to each other. I say *real straight lines,* to distinguish them from those straight lines of which a curve is hypothetically conceived to be composed, for purposes of mathematical calculation.

These dynamical lines have real magnitude, and their magnitude varies with the intensity of the force and with the velocity of the moving body.

For, since we have proved in Sect II, Book II that a force acts in time and that its effects are not continuous, but impulsive, therefore, during the *microt,* when no action of the force takes place, the body moves freely by its motional inertia only; that is to say, in a straight line. The magnitude of the straight line for any given velocity depends on the magnitude of the microts; and since the microts are inversely proportional to the intensity of the force, therefore, is the magnitude of theses traight lines, also, inversely proportional to this intensity; and when the force is very small they must be comparatively large. (In the elliptical motion of comets, for instance, when they are at a large distance from the sun, these straight lines, I take the liberty to assume, must be measured by miles.) At the end of the microt a sudden action takes place, and the body moves along the well known diagonal in a straight line until another sudden action takes place; and so on and on.

Remark. In our introductory remarks to this book we introduced several problems to be considered, one of which (the 2d) was: "What is the law of motion when two given quantities of energy act on a body simultaneously in different directions?"

Of this question we disposed briefly, far from having done the subject justice, in the first two lemmas, and in the first three proposition. The 4th, 5th, 6th, and 7th Propositions were intended as preparatory to our handling of the remaining questions, among which we will pay particular attention to the 1st question. As we are now ready with an answer to the remaining questions, we wish to remark that when we say: *What is the energy value of the 2-d component?,* we mean, what is the amount of energy that must be exerted in order to produce the velocity of the second component, but not the actual kinetic energy of motion in the direction of the 2d component.

Lemma 3. The amount of energy required for the production of any (molar) effect on a body in motion (or at rest) is independent of the nature of the cause producing that (molar) effect, and is, therefore, the same whether its cause be finite energy or an infinite force—gravitation.

Lamma 4. The amount of energy required for producing any effect whatever upon a body, must of necessity appear in the body (on which the effect was produced), either as molar energy or as molecular, irrespective of the cause of action, whether finite or infinite. For the effect, after its production, can have no relation to its past cause; and, therefore, the same effect always represents and is accompanied by the same amount of energy, no matter what its cause. (We take it for granted that a finite energy spent for producing any effect upon a body must appear in that body; for a finite energy cannot be annihilated.)

There is one case, however, where the amount of energy present in the body acted upon differs with its cause: whether it be finite or infinite. It is this: If a molar effect be produced on a body in motion; and if the molar effect produced be contrary in direction to the original motion; and so partly or wholly destroy the original motion—then, the kinetic energy of the part so destroyed and the energy required for effecting this destruction are not annihilated, if the cause of the molar effect be a finite energy; for, a finite energy can neither create nor annihilate motion. The kinetic energy destroyed and the energy required to destoroy the motion must, therefore, in the case of a finite cause, appear in the body in the form of molecular motion—heat. On the other hand, if the cause producing the destruction of the original motion be an infinite force—gravitation—then, since gravitation is a force capable of creating and annihilating motion, the energy of motion thus destroyed is entirely and absolutely annihilated, without any form of molecular energy taking its place. In the language of physicists, the kinetic energy thus destroyed by gravitation, is said to have been converted into potential energy. (Be it so. We have discussed the subject at length in Sect. II, Book II. For our present purpose we may allow ourselves to view the energy destroyed as having been converted into potential energy.)

PROPOSITION 8. If a velocity V_1 be communicated in any direction to a body (of unit mass) while moving with a velocity V in a different direction, then, will the diagonal of the well-known parallelogram represent the direction and velocity of the resulting motion.

Demonstration. This has been proved apriorily in Sects. I and II of Book III. Newton's mode of proving the law of *parallelogram forces,* although it is, as we have shown, erroneous, because of the inaccurate measure of an impulsive force and because of the erroneous assumption that a definite amount of energy will produce the same molar effect on a body, whether it be in motion or at rest—is still perfectly applicable to this proposition, if we substitute *velocity* in place *of force.* For, in this proposition we have nothing to do with the forces involved nor with the amount of force necessary to produce the second component.

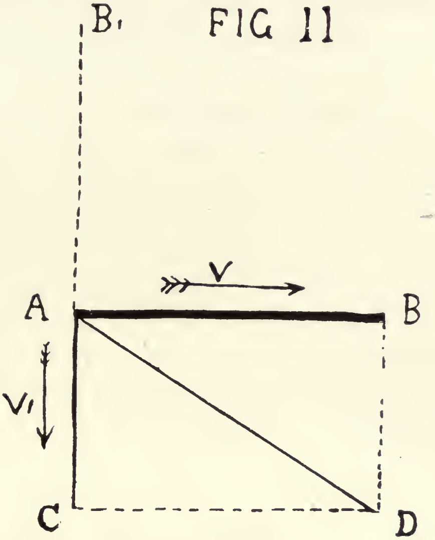
Aposteriorily, this proposition is proved from the motion of the heavenly bodies, as we have repeatedly stated.

PROPOSITION 9. If a velocity V_1 be impulsively communicated to a body moving by inertia with a velocity V; and if the direction of the velocity V_1 be perpendicular to the direction of the original motion of the body; then, we say, that the amount of energy required to produce the velocity V_1 is equal to the amount of energy that would be required to increase the velocity of the body from V to $V + V_1$, were the body moving in the direction of the velocity V_1.

In Figure 11, let AB (=V) represent in magnitude and direction the original motion of the body. At A, draw the perpendicular B_1C; make B_1A equal to

AB ($=V$), and AC equal to V_1; AC, then, will represent in magnitude and direction the velocity V_1. And we say, that the amount of energy required to produce the velocity AC ($=V_1$), is equal to the amount

FIG 11



of energy that would be required to increase the velocity of the body from $B_1A = AB = V$ to $B_1C = V + V_1$, were the body moving in the line B_1C, in the direction of the arrow, with a velocity $B_1A = AB = V$.

Demonstration. Since AC is perpendicular to AB. and since the velocity $AC = V_1$ is away from the line of motion AB, therefore, by proposition 5, it is in the direction of the directional inertia on that side; and by proposition 6, the force producing the velocity V_1 meets at the point A, a directional resistance equal to V. But this would be the amount of motional resistance that the same force would meet with, were the body moving in the line B_1C, with a velocity $B_1A = V$. (Prop. 8, Sect. III, Book II.) Further, when any small part (x) of the velocity V_1 has been already communicated to the body moving along the line AB, with the velocity V, the resistance that the cause of the velocity V_1 meets with, is, then, composed of two parts: the directional resistance equal to V, and the motional resistance due to the velocity x in the direction of AC; the whole resistance, therefore, would be equal to V, directional resistance + x, motional resistance; or, (since the nature of the resistance does not signify as to the amount of energy required for overcoming it) the whole resistance would, then, be equal to $V + x$, the same as the cause of the velocity V_1 would meet with, were the body moving along the line B_1C ,with a velocity V, after it has communicated to it an additional velocity x; for, then, the velocity of the body would be $V + x$, and its motional inertia would, therefore, also be equal to $V + x$.

It is thus seen, then, that throughout the communication of the velocity V_1, the resistance to be overcome by the force, when the body is moving in line AB, with a velocity V, is equal to the resistance that would have to be overcome, if the velocity V_1 were to be communicated to the same body, if it were moving along the line B_1C, with a velocity $V = B_1A$.

Therefore, the whole resistance to be overcome for the production of velocity V_1, when the body is moving along AB, is equal to the resistance to be overcome for the production of the same velocity V_1, when the body is moving along B_1C, provided that the original velocity of the body be supposed to be the same in both cases.

But for overcoming equal resistances, equal amounts of energy are required: hence our *proposition.*

Corollary. Since, when the velocity of a body of unit mass is increased from V to $V+V_1$ its energy is increased by $\frac{1}{2}(V+V_1)^2 - \frac{1}{2}V^2 = VV_1 + \frac{V_1^2}{2}$ we, therefore conclude, by lemma 4, that the amount of energy required for the production of this increase is also equal to $VV_1 + \frac{V_1^2}{2}$; and, therefore, by this proposition, we conclude that the amount of energy required to produce a velocity V_1 perpendicular in direction to the inertial motion of the body with a velocity V, is, also, equal to

$$VV_1 + \frac{V_1^2}{2} \text{ (for unit mass) } \ldots\ldots \quad (1)$$

PROPOSITION 10. The VV_1 of the formula (1) of the corolary to proposition 9, represents the amount of energy required to overcome the directional inertia of the body, due to the inertial motion of the body along the line AB, in Figure 10; and the $\frac{V_1^2}{2}$ in the same formula represents the amount of energy required for overcoming the motional inertia arising from the velocity V_1, itself.

Demonstration. Let the microf be the unit of velocity; then V and V_1 will represent the number of microfs of which the velocities V and V_1 are composed.

Now, the energy required for producing the first microf along the line AC will be equal to V for the directional resistance and to (1) for the motional resistance; the energy required for the production of the second microf will be equal to V, again, for the directional inertia, and to (2) for the motional inertia; for the third microf, to V, again, for the directional resistance, and to (3) for the motional inertia; and so on; for the last, that is to say the V_1 microf in the series, the amount of energy required will be equal to V, again, for the directional inertia, and to V_1 for the motional inertia.

Put all this into the following series:—

For	1st	m'f......	V	(D. R.)	+ 1	(M. R.)
"	2d	"	"	"	+ 2	"
"	3d	"	"	"	+ 3	"
"	4th	"	"	"	+ 4	"
"	——	"	————			
"	——	"	————			
"	V_1	"	V	(D. R.)	+ V_1	(M. R.)

(D. R.) means directional resistance, and (M. R.) motional resistance.

Sum up the column of D. R. and the column of M. R., separately, and we find that the sum of the column D. R. is equal to VV_1; and also that the sum of the column M.R. is equal to $\frac{(1+V_1)V_1}{2}$ and, since V_1 is a very large number for any sensible velocity, $1 + V_1$ may be considered equal to V_1; the sum of the column (M. R.), therefore, is equal to $\frac{V_1^2}{2}$

Q. E. D.

Scholium. It seems to me to be self-evident, that only energy spent for overcoming motional inertia can possibly, and must, appear as molar energy in the resulting motion of the body along the diagonal AD (Fig. 11); and, that the energy spent for overcoming directional inertia cannot possibly appear as molar energy in the resulting motion of the body. Therefore it is, that the velocity of the resulting motion is equal to AD, and its kinetic energy is equal to $\frac{AD^2}{2}$; for, $\frac{AD^2}{2}$ is equal to $\frac{AB^2}{2}+\frac{AC^2}{2}$, or

$$\frac{AD^2}{2}=\frac{V^2}{2}+\frac{V_1^2}{2}$$

from which equation we see that the energy of the resulting motion is equal to the original kinetic energy of the moving body, plus the energy required for overcoming the motional inertia, due to the velocity V_1; while VV_1, the energy spent for overcoming directional resistance, does not appear as molar energy in the resulting motion. Nor could it, as it appears to my mind, possibly appear as molar energy, since VV_1 was spent not for producing motion, but for something else, namely, for producing change of direction.

PROPOSITION 11. The energy VV_1 of formula I, in the corollary to proposition IX, spent, as we have proved in Proposition X, for change of direction, must appear in the moving body as molecular energy—heat; whether the cause of the second component V_1, be a finite force, such as light, heat, electricity, magnetism, chemical affinity and molar motion; or it be an infinite force—gravitation.

Demonstration. In the case of a finite force this is clear and positive. For the finite energy VV_1, *has been spent* for change of direction (Prop. VII.); and since a finite energy can positively not be annihilated, nor can it annihilate anything, therefore, it must appear in the moving body either as molar or as molecular energy. (See Lemma IV.). And since, as we have proved in the 10th proposition, it can not and does not appear as molar energy in the resulting motion along the line AD (figure 11), therefore, it must, of necessity, appear in the moving body as molecular energy, and that is heat. So far, for a finite force.

In case of an infinite force, this proposition is proved by the aid of Lemmas III and IV. For, by Lemma III we infer that the energy VV_1, for change of direction, must have been *actually* spent for producing change of direction, even in case of an infinite force; and since it *has been spent,* it must, by Lemma IV, appear in the moving body, either as molar or as molecular energy; and since it does not appear as molar energy, it must appear as molecular energy—*heat.* I say *heat,* for from nature we learn that the most common form of molecular energy connected with molar motion is *heat;* as when the motion of a body is arrested, or when motion results against any sohesive resistance,—friction, for instance.

Scholium. It appears to my mind, that the demonstration of this proposition, even with respect to an infinite force, is absolutely conclusive. So, I firmly hope, it must appear to every unbiassed reader, who will carefully analyse the chain of reasoning, started, I may say, on the 1st page of this book and ending in this proposition. Nothing, as far as I can see, has been arbitrarily assumed for the purpose of building up a theory. On the contrary, everything, I persuade myself, has been rigorously proven, either apriorily or aposteriorily, or by both methods, or if assumed, is so self evident that its denial is an impossibility.

But, because of the vast importance of this proposition, as far as an infinite force is concerned, as will presently appear evident, I will add the following consideration:

The only possible ground on which a doubt may be raised to this proposition, as far as gravitation is concerned, by any skeptic mind, is the fact that gravitation is, admittedly, a force capable of annihilating motion; as in the case of gravitation acting *against motion.* And while so annihilating motion, the force, or the exertion, or the energy required to effect this annihilation is itself annihilated, and does not appear in the body so acted upon by gravitation. It may, therefore, be argued and said, "you are perfectly right, as far as a finite force is concerned, since a finite force cannot annihilate anything, and cannot be annihilated itself; but as for an infinite force, is it not possible that gravitation, while overcoming directional resistance, annihilates that resistance; and while so annihilating the resistance, the energy exerted to effect this annihilation is itself annihilated; the two—the directional resistance and the energy required for overcoming it—counteracting each other, and becoming zero; just as is the case, when gravitation acts *against* motion?

So the skeptic may argue. But, I say, that this mode of reasoning is entirely unreasonable; for, the directional inertia is primarily due to the motion of the body along the line AB (Figure 10), and is, therefore, an inseparable property of that motion, and cannot, therefore, be conceived to be annihilated without simultaneous annihilation of that motion of which it is a property. Therefore, as long as the motion along line AB continues, that is to say, during (or in) the composition of the two velocities V and V_1, and before, so to speak, motion takes place along the line of the resultant, AD,—the directional resistance must persist and cannot be annihilated: it may only be overcome, but not annihilated. In the case of the static circle, too, mentioned in Prop 7, we are far from saying, that the cohesive resistance annihilates the directional inertia; we only affirm that they balance each other; and this is far from being the same as "annihilate each other": The static circle is, merely, an equilibrium circle, where the two resistances equlibrate each other, both being continually present.

And if the resistance of direction cannot be annihilated, the energy required for overcoming it can also not be annihilated. The energy is merely *spent* for producing an effect, that is, change of direction or what is the same thing, for overcoming directional inertia, but not annihilated. It must, thereforee, appear in the moving body as energy of some form.

But I fear that the mind of the extremely stubborn skeptic will still not be satisfied, and he may perhaps discern other doubts, and doubts behind doubts, as are the picttures in a kaleidoscope, each causing the other, even to infinity; especially, because of the startling novelty of the assertion. (For it is not to be denied that the assertion that a flying stone pulled out of its course by a pull on the rope attached to it, has, in consequence of the pull out of its line of motion, heat deceloped throughout its substance—is both extremely novel and startling. Doubly so, when it is asserted, that a stone projected horizontally, and which, in consequence of gravity, describes a parabola, or, rather, a part of a parabola, has, also, heat developed in its substance because of its continually being pulled out of the right line of motion by gravitation! !)

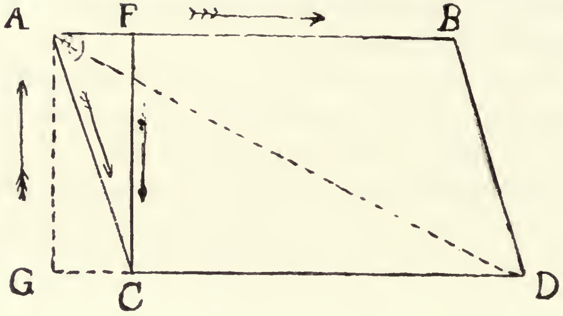
But such is the inevitable and necessary conclusion. As for the extreme skeptic, the only remedy is to produce an aposteriori proof; in other words, to bring forward facts from nature that cannot be otherwise satisfactorily explained than by the acceptance of our proposition as true and correct.

Such facts and such proofs are of an overwhelming abundance in nature; and we do not have to go far to look for them, as we shall presently see.

Experimental proof is hardly to be looked for, for this proposition, as far as gravitation is concerned, as the reader may easily comprehend. For, the velocities, with which we can possibly perform our experiments, are comparatively small, and, therefore, VV_1 is, also, comparatively small; and the amount of heat developed by VV_1, will, therefore, be a small fraction of a degree only. I do not say that experimenting for the purpose of verifying the correctness or falsity of this proposition is an impossibility; but I say that it is extremely difficult, and must involve perserverence and expenditure. And my strongest wish, I confess, is to have such experiments performed; for I am firmly convinced that the results arrived at would most satisfactorily prove the correctness of our assertion.

PROPOSITION 12. If a body be moving with a velocity V in any direction, and, while so moving, a velocity V_1 be impulsively and at once communicated to it in any direction whatever, the two directions making an angle (a); then, is the energy required for producing the second velocity V_1 equal to the energy required for producing a change of the velocity in the line of the inertial motion, and equal to $V_1 \cos a$; plus the energy required for producing a velocity in the body, in a direction perpendicular to the line of motion, and equal to $V_1 sna$.

FIG 12.



In figure 12, let AB be the line of the old inertial motion; make AB equal to V. Let AC represent in magnitude and direction the second velocity V_1; let, also, the angle BAC be equal to angle (a). Drop the perpendicular CF. Then, we say, that the energy required for producing the velocity AC=V_1, is equal to the amount of energy required for producing in the moving body of a velocity AF=$V_1 \cos a$ along the line of motion AB, plus the energy required for producing in the moving body a velocity FC= V_1 sn a in a direction perpendicular to the line of motion, as indicated in the figure above.

Demonstration. At A draw the perpendicular AG; the line AG, in the direction of the arrow, will represent the direction of the directional resistance, by proposition IV. The energy or force producing the velocity V_1 will, therefore, meet at the point A, with two resistances, namely, the motional resistance along the line AB and the directional resistance along the line AG; but it will meet with no other resistance. The energy, then, required for producing the velocity V_1

is entirely spent for overcoming these two resistances and for nothing else. But by lemma I. a body moving with a velocity V_1=AC in the direction AC from A to C will move in the direction of AG from A to G with a velocity AG=FC=V_1 sn a; a certain amount of energy will be spent for producing the velocity AG, since it is in the direction of the directional resistance; this amount of energy denote by E'AG. Similarly, by lemma I., a velocity equal to AF=V_1 cos a will be communicated to the body in the direction of AB. But AB is the line of motional resistance, and, therefore, a certain amount of energy will be required for overcoming the motional resistance, and thus producing the velocity AF. This amount of energy denote by E' AF. The sum of the two: E' AG + E' AF must be equal to the energy required for producing the velocity AC, since there is no other resistance but the motional resistance along AF and the directional resistance along AG.

Therefore, denoting the amount of energy required for producing the velocity AC by E' AC, we get the following equatation:

$$E'AC = E'AG + E'AF \qquad \text{(II.)}$$

Substituting the values of AC, AG, and AF, in formula II., we get—

$$E'V_1 = E'V_1 \cos a + E'V_1 \text{sn} a \quad . \quad . \quad . \quad . \quad \text{(III)}$$

which proves our proposition.

Corollary 1. Since the change of velocity AF=V_1 cos a, is in the line of motion, AB, and since the original velocity is V, therefore, is the amount of energy required for producing the velocity $AF = V_1 \cos a$ equal to

$$\frac{[V + V_1 \mathrm{Cos}(a)]^2}{2} - \frac{V^2}{2} \text{ or } E'AF = E'V_1 \cos(a) =$$

$$\frac{V_1^2 \cos^2(a)}{2} + VV_1 \cos(a) \qquad \text{(IV.)}$$

Further, the amount of energy required to produce the velocity $AG = FC = V_1 \mathrm{sn}(a)$ is by proposition II. equal to

$$\frac{V_1^2 \mathrm{sn}^2(a)}{2} + VV_1 \mathrm{sn}(a)$$

$$\text{or } E'AG = E'V_1 \mathrm{sn}(a) = \frac{V_1^2 \mathrm{sn}^2(a)}{2} + VV_1 \mathrm{sn}(a) \, .. \, \text{(V.)}$$

The sum of equations IV. and V. is

$$E'AF + E'AG = \frac{V_1^2 \cos^2(a)}{2} + VV_1 \cos(a) +$$

$$\frac{V_1^2 \mathrm{sn}^2(a)}{2} + VV_1 \mathrm{sn}(a)$$

$$\text{or} \quad E'AF + E'AG = \frac{V_1^2}{2} + VV_1 \cos(a) + VV_1 \mathrm{sn}(a)$$

or, since E'AC is equal to E'AF + E'AG, by equation II., therefore,

$$E'AC = \frac{V_1^2}{2} + VV_1 \cos(a) + VV_1 \mathrm{sn}(a) \qquad \text{(VI)}$$

Corollary 2. If to both sides of equation VI. we add $\frac{V^2}{2}$, the amount of energy of the original inertial motion V, wet get

$$E'AC + \frac{V^2}{2} = \tfrac{1}{2}\,[V^2 + V_1^2 + 2VV_1 \cos(a)] + VV_1 sn(a) \qquad \text{(VII.)}$$

But $\tfrac{1}{2}\,[V^2+V_1^2+2VV_1\cos(a)]$ is equal to $\frac{AD^2}{2}$ AD (figure 9) being the velocity of the resultant; therefore

$$E'AC + \frac{V^2}{2} = \frac{AD^2}{2} + VV_1 sn(a) \qquad \text{(VIII.)}$$

Now, as $E'AC + \frac{V^2}{2}$ represents the whole energy that must appear in the body; and as $\frac{AD^2}{2}$ is the amount of kinetic energy of the resultant, therefore, we conclude that $VV_1 sn(a)$, a part of E' AG by equa- (V.), does not appear in the body as molar energy of the resultant, and must, therefore, appear in the moving body as molecular energy or as *HEAT;* and that, whether the force be finite or infinite as proved in propositions 10 to 11. For the amount of energy VV_1 sn(a) is a part of E' AG, and $AG=V_1 sn(a)$ is the velocity communicated to the moving body in a direction perpendicular to the line of motion. Further, $VV_1 sn(a)$ is, by proposition X., that part of E' AG which has been spent for overcoming directional resistance (D. R.), and by scholium to proposition XI., cannot appear as molar energy.

Corollary 3. As sn(a) is equal to sn(180°—a), therefore VV_1sn(a) is equal to VV1sn(180—a); and we infer that the amount of heat developed, when a velocity V_1 is communicated to a body in motion with a velocity V, is the same whether the direction of the two velocities make any angle (a) or its complement (180°—a). (See corollary 4.)

Corollary 4. AF is equal to V_1cos(a); and as a cosine varies from + 1 to — 1, therefore, V_1cos(a) may be either positive or negative. It is positive when angle (a) is less than 90°, as in figure 11; and it is negative when the angle between the direction of the velocities is more than 90°.

When AF is negative, the velocity communicated along the line AB is contrary in direction to the original velocity V, and destroys part of the original velocity; and if the force producing the velocity V_1 be a finite force, the energy of the part of velocity V destroyed together with E'AF must also appear in the moving body as molecular energy; so that the total amount of heat developed in such cases is equal, in units of energy, to

$$VV_1\mathrm{sn}(a) + 2\ E'AF \qquad \text{(IX.)}$$

But, (and this is important), if the force producing the velocity V_1 be gravitation, then, in the case of an obtuse angle the part of the original velocity V destroyed and E'AF are entirely annihilated and therefore do not appear in the moving body as heat. So that the amount of heat developed when a velocity

V_1 is communicated by gravitation to a body moving with a velocity V, is under all circumstances equal, in units of energy, to $VV_1\text{sn}(a)$ only, no matter what the magnitude of the angle between the directions of the two velocities.

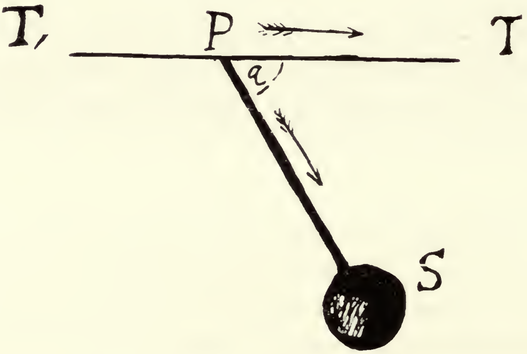
Remark. All the quantities of energy mentioned throughout the preceding proposition, have referenece to a unit of mass; and for a mass M must, therefore, be multiplied by M.

PROPOSITION 13. In the revolution of the planets and comets around the sun, no matter in what form of orbit, whether circular or elliptical, heat is constantly being developed throughout their substance. And the amount of heat developed per unit of time and for each unit of their masses, if expressed not in degrees, but in units of energy, is expressed by the formula.

$$FV\text{sn}(a) \qquad \textbf{(X.)}$$

where F denotes the intensity of the sun's gravitation on the planet or comet for the unit of time (second) under consideration, and V denotes the orbital velocity of the planet for the same unit of time, and which, for simplicity, we consider constant for that unit of time (second); and (a) denotes the angle made by the direction of gravitation with the direction of the orbital motion of the planet at that time, that is to say, with the tangent to the orbit; reckoning the angle in he direcion of the motion of the planet and not in the contrary direction, as indicated in figure 13, where S is the sun, P the planet, PS the direction of gravitation, T_1PT the tangent to the orbit at point P, and the planet moving in the direction of the arrow; the angle (a) is the angle SPT, but not the angle SPT_1. The angle (a) is also supposed to be constant for that unit of time (second).

FIG. 13



Demonstration. Gravitation acts impulsively and communicate any given velocity to a body, not at once, but in succession, by microfs; an interval of time, a microt, separating each accretion of the small velocity from the other. (Prop. III., Sect. III., Book II.) Take the microf as the unit of velocity; then F will represent the number of microfs which the force is capable of communicating to a body in the unit of time; it will also be the number of microts in that unit of time. (V will also represent the number of microfs of which the original velocity is composed.)

Now, on the production of a microf (the unit of velocity) by pravitation, a certain amount of directional inertia is overcome, part of the energy for overcoming which does not appear as molar energy in the resultant, and must, therefore, appear as *heat* in the moving body. This part is represented by VV_1sn a (in equation VIII. of proposition XII., corollary 2).

But V_1 in this case is equal to one, the microf being the unit of velocity; therefore, VV_1sn a becomes in this case

$$V\text{sn } a \qquad \text{(XI.)}$$

We, therefore, conclude that on the production of each microf, that is to say, during one microt, an amount of heat energy equal to Vsna is developed in the moving body; and as there are F microts in the unit of time (the second), the amount of heat developed in the unit of time will, therefore, be F times as large; that is to say, it will be

$$FV\text{sn } a \qquad \text{(XII.)}$$

which proves our proposition.

Corollary 1. If the orbit of the planet be circular, or nearly so, as is the case with the earth's orbit, then, is the angle (a) equal to 90°, and its sinus equal to unity; and formula (XII.) becomes

$$FV \qquad \text{(XIII.)}$$

for all points of the orbit; that is to say that the amount of heat developed in a planet revolving in a circular orbit, is, for any definite time, a constant quantity.

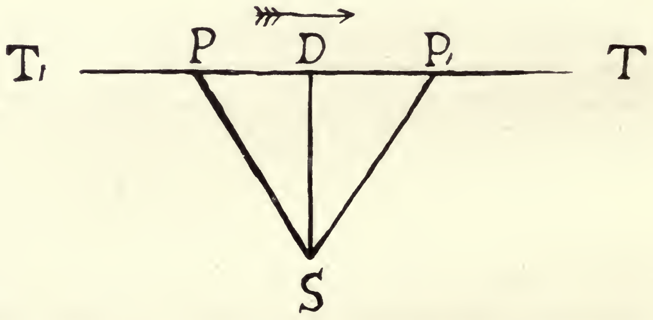
Corollary 2. Since for a circular orbit F, the centripetal force, is equal to $\frac{V^2}{r}$ or $F = \frac{V^2}{r}$, where r is the radius of the orbit; therefore, substituting $\frac{V^2}{r}$ for F in equation (XIII.) we get

$$\frac{V^3}{r}\,; \qquad \text{(XIV.)}$$

that is to say, the amount of heat developed in a circular orbit, in a unit of time, is directly proportional to the cube of the velocity of the planet, and inversely proportional to the radius of the orbit.

PROPOSITION XIV. The amounts of heat developed per unit of time in a planet revolving around the sun in an ellipictical orbit, in different parts of the orbit —are inversely proportional to the cube of the radii vectors of these parts.

FIG. 14



Demonstration. In figure 14, let T_1T be the tangent to a point P of the orbit; P the planet; let S be the sun. Let PP_1 ($=V$) be the orbital velocity of the planet; PS then will be the radius vector at the point P, which denote by R. Drop the perpendicular SD. Connect S and P_1 by a straight line. In the triangle SPP_1 thus formed, the angle SPP_1 will be the angle (a) of formula (X.).

Now, the triangle SPP_1 will, if the unit of time be small, represent the area described by the radius vector in the unit of time, which, as is well known, is the same for all parts of the orbit, and is, therefore, a constant quantity. Denote this constant quantity by C. The area of the triangle SPP_1 will, therefore, be equal to C. But the area of the triangle SPP_1 is equal $\frac{PP_1 \times SD}{2}$; we, therefore, get the equation

$$C = \frac{PP_1 \times SD}{2} \qquad \text{(XV.)}$$

But PP_1 is equal to V, the orbital velocity of the planet; and SD is equal to Rsn(a). (R being the radius vector PS.) Substitute these values for PP_1 and SD in (XV.) and we get

$$C = \frac{VRsn(a)}{2} \qquad \text{(XVI.)}$$

Now, denote the heat-energy developed in the unit of time by E; and, since by proposition (XIII.) it is equal to FVsn(a), we, therefore, get the equation

$$E = FVsn(a) \qquad \text{(XVII.)}$$

Since the point P has been taken at random, equations XVI. and XVII. are true for any part of the orbit.

Now divide equation XVII. by XVI. and we get

$$\frac{E}{C} = \frac{2F}{R} \qquad \text{(XVIII.)}$$

Now as F is inversely proportional to R^2, we may put F equal to $\frac{C_1}{R^2}$, where C_1 is a constant; and we get

$$\frac{E}{C} = \frac{2C_1}{R^3} \qquad \text{(XIX.)}$$

or $$E = \frac{2CC_1}{R^3}\ ; \qquad \text{(XX.)}$$

from which equation (XX.) we infer that E is inversely proportional to the cube of R, since C and C_1 are constants.

PROPOSITION XV. The amount of heat-energy developed in a planet revolving in a circular orbit, during a complete revolution—is equal to the circumference of its orbit multiplied by the intensity of solar gravitation on the planet.

Demonstration. By equation XVIII. we have $E = \frac{2CF}{R}$; that is to say the amount of heat-energy developed by a planet revolving in any orbit, in a unit of time is equal to $\frac{2CF}{R}$, where C is the area described by the radius vector for that unit of time and F is the intensity of solar gravitation in the planet and R is the radius vector.

Now, in a circular orbit C, F, and R do not change; therefore, E also does not change; that is to say, the amount of heat-energy developed per unit of time is the same in all parts of the orbit.

Now, let T be the periodic time of the planet; TE will then be the amount of heat-energy developed during a complete revolution. But TE is equal to

$$T\left(\frac{2CF}{R}\right), \qquad \text{or}$$

$$TE = \frac{2TCF}{R} \qquad \text{(XXI.)}$$

Now, as C is the area described by the radius vector in a unit of time, TC is the entire area of the orbit, which is, therefore, equal to πR^2.

Putting πR^2 in place of TC in equation XXI. we get

$$TE = \frac{2\pi R^2 f}{R}, \qquad \text{or}$$

$$TE = (2\pi R)F \qquad \text{(XXII.)}$$

Q. E. D.

SCHOLIUM TO PROPOSITION X.—XIV.

The various formulae of the preceding propositions having reference to the amount of heat-energy developed in the planets while revolving in their orbits, have, and can only have, in the present state of our knowledge, a relative value, but no absolute value. I mean, that by the aid of these formulæ we may with exactness determine, firstly, the proportion of the amount of heat-energy developed in a planet in a definite interval of time, in one part of the orbit, as compared to the amount of heat-energy developed in the same planet during the same interval of time, in a different part of the orbit; secondly, the proportion of the amount of heat-energy developed in a given planet during a definite interval of time in a definite part of its orbit, as compared to the amount of heat-energy developed in another planet during the same time in a definite part of *its* orbit; and, thirdly, many other relations, as the reader will understand for himself.

But these formulae can have, in the present state of our knowledge, no absolute value. I mean that these formulae can not, at present, be employed for determining the absolute amount of heat developed in a revolving planet, as expressed in degrees or in calories; that is to say, in other words, that the thermal equivalent of Meyer and Joule, or the quantitative relation between molar energy and heat, as determined by Meyer and Joule, here, on our earth, can be applied only to all motions on the earth alone, but not to the motions of the planets themselves, including our earth; nor to the proper motio nof the sun; nor to that of the stars.

Physicists and astronomers generally delight in making statements like the following: If the orbital motion of this or that planet be suddenly arrested, then, would the temperature of the planet be raised to such and such a degree, and the amount of heat produced at the expense of the kinetic energy thus arrested would be such and such. Again, they say, the planet whose orbital motion has been thus arrested would fall in a straight line to the sun, and the amount of heat produced by the collision of the planet with the sun would be such and such. And they calculate all this by applying the thermal equivalent of Myer and Joule to the velocities possessed or acquired by the planets, relative to the sun considered as at rest. For the physicists hold that all motion is relative, and, therefore, they can see no differnce, as far as thermal energy represented by motion is concerned, between one relative motion and the other: between the motion of the bullet shot from a gun, which is relative to the earth considered as at rest, and the motion of a planet relative to the sun considered at rest. And, therefore, they apply the thermal equivalent of the one to that of the other. (See definition. Book II., Sect. III.)

But since the physicists fail to offer any positive proof of the identity of the two motions as far as the thermal energy represented by them is concerned, we may well doubt it. And with perfect reason. For the state of rest to which the motion of the bullet is referred is entirely different from the state of rest to which the motion of the planet, say the earth, is referred. The state of rest of the bullet is that of the earth, of which the bullet (before and after motion was communicated to it) forms a part. But the state of the earth is really not a state of rest. In the first place it possesses the motion of rotation; secondly, the orbital motion around the sun; thirdly, together with the sun, the proper motion of the solar system. This complicated motion, then is the state of rest to which the motion of the bullet is referred.

In the other case, that of the motion of the earth around the sun, the state of rest to which the earth's orbital motion is referred, is that of the sun; and the sun, assuredly, does not possess the first and second motions enumerated above in case of the earth.

Therefore, I claim that the first microd of the motion of the bullet is by much larger than the first microd of the motion of the earth around the sun. In other words, the amount of energy required to produce the first microf of the bullet by terrestial forces is much larger than the amount of energy required to produce by forces within the solar system a first microf in a body on the sun, or in a body moving together with the sun and accompanying the sun in its proper motion. For the bullet at rest is possessed of the three motions enumerated above; while the solar body at rest possesses only one of those motions.

And, if (as we have proved in proposition VIII., Sect. III., Book II.) an increase of motion in a moving body causes an increase of resistance to further motion, we have perfect reason to infer by analogy that an increase of the motion constituting the state of rest of a body likewise causes a greater resistance to motion, and, consequently, an increase of the first microd. And since by Prop. VIII., Sect. III., Book II., the total kinetic energy of a body is a summation of all its microds; and since, by the same proposition the magnitude of the microds varies and is proportional to the serial number of the microds; therefore, it follows that, if the magnitude of the first microd be increased, the total kinetic energy of the body for a given velocity will also be proportionally increased; and if diminished diminished. That is to say, the total kinetic energy of a body moving with a given velocity is proportional to the magnitude of its first microd. And, therefore, if the magnitude of the first microds differs in differnet systems of bodies, the amount of kinetic energy (as measured by heat) of a given velocity in one system of bodies will differ from the amount of energy of the same given velocity in another system of bodies; and the two amounts will be proportional to the magnitudes of their respective first microds. Therefore, the thermal equivalent of one system will differ from the thermal equivalent of another system, and the two will be proportional to the magnitudes of the first microds of the respective systems.

Now, the earth (the moon) and everything on the earth, including the bullet, constitute one system of bodies; the solar system, including the earth, constitute another system. And, therefore, the thermal equivalent of the motion of the bullet differs from the the thermal equivalent of the orbital motion of the earth; and the two equivalents are proportional to the magnitudes of the respective first microds of the two systems.

What the actual value of this proportion is, that is to say, what the value of the thermal equivalent of the solar system is—is at that present state of our knowledge impossible to determine, as will become obvious to anyone carefully considering the insurmountable difficulties of the problem, which I will not stop to enumerate.

For all we know, the thermal equivalent of the solar system may be, and probably is, as we will perhaps prove further on, hundreds of thousands of times less than the thermal equivalent of Myer and Joule.

Even for the terrestial system, I do strongly believe that the thermal equivalent of Myer and Joule varies with the orbital motion of the earth around the sun; for any change of that motion will constitute a change in the state of rest to which terrestial motion is referred. Therefore, I believe that the thermal quivalent is slightly larger during the winter months, when the earth is near the perihelion and its orbital velocity near its maximum, than during the summer months with the earth near the aphelion and near its minimum of orbital velocity.

in different series of experiments different values for his equivalent, varying from 770 foot-pounds to 787 foot-pounds; and he was, therefore, as it seems, not satisfied with any of them. He again and again reattacked his beloved problem and re-experimented even at intervals of many years with hardly better success; and was, finally, compelled to take the average equivalent of all his experiments as the one nearest to the truth.

Different experimenters, also, have achieved different values of the equivalent. Why all this discrepency, I ask? Is not this fact suggestive of what I said above that the thermal equivalent varies with the velocity of the orbital motion of the earth, and therefore, have the various experimenters, taking no notice of the time of the year in which the experiments were performed, arrived to different and apparently conflicting results? I say this merely as a suggestion. It is greatly to be regretted, though, that the dates of the different experiments by the different men have not been recorded.

I have thus spoken at length of this subject, because of its importance for our purpose. Were we not to take cognizance of this and were we to apply the terrestial thermal equivalent to the motions of other systems, we would get greatly disappointed at the results arrived at by our frumulae. Take, for instance, formulae XIII. of corollary to proposition XIII., and apply it to the motion of the earth around the sun: F, in this case, is equal to $\frac{20^2}{95,000,000}$ miles (we take the earth's orbital velocity as equal to 20 miles), or $\frac{1}{237,500}$ miles; V is equal to 20; FV, therefore, is equal to

$$20 \times \frac{1}{237,500} = \frac{2}{23,750} = \frac{1}{11,875} \text{ miles.}$$

As our measure of energy is by ½ the square of the velocity, $\frac{1}{11,875}$ miles represents ½ the square of some velocity x; or

$$\frac{1}{11{,}875} = \frac{X^2}{2}\text{; or } X^2 = \frac{1}{5937{\cdot}5}\text{, or}$$

$$x = \sqrt{\frac{1}{5937 \cdot 5}} = \frac{1}{\sqrt{5937 \cdot 5}} = \frac{1}{77+} \text{ miles, or}$$

$$x = \frac{6280}{77} \text{ feet.}$$

That is to say that the heat-energy developed in each unit of mass of the earth's substance in one second is equivalent to the kinetic energy of a body (of a unit of mass) moving relative to the sun as at rest, with a velocity of 67 ⅓ feet. In other words, if the motion of mass) moving, relative to the sun as at rest, with a velocity of 67 ⅓ feet be arrested and its kinetic energy thus be converted into heat, the amount of heat thus developed will be equal to that which the earth has developed in each unit of mass of her substance in one second.

Now, on the earth, a velocity of 67⅓ feet in a body of a unit (pound) of mass, if arrested, will be converted in an amount of heat equal to $\frac{(67\frac{1}{3})^2}{(222)^2}$ calories nearly or a little less than $\frac{1}{11}$ of a calorie (foot-pound Fahrenheit).

For the thermal equivalent of Myer and Joule is about 775 foot-pounds which is equivalent to the kinetic energy of a body of unit of mass moving with a velocity of 222+ feet.

Now, were we to apply this terrestial thermal equivalent to the orbital motion of the earth, the earth would, according to this, have developed in every pound of her substance $\frac{1}{11}$ of a calorie per second or one calorie in 11 or 12 seconds, which is an exceedingly large quantity (as will be obvious to any one who is at all acquainted with the subject).

Therefore, it is that we have so insistantly throughout this cholium endeavored to defend our position by drawing attention to the distinction, with regard to the thermal equivalent, between the motion of one system and that of another system, between the motion of a body on the earth and the motion of the earth in her orbit.

And we hold that this quantity of $\frac{1}{11}$ of a caloric per second, in the earth's case, ought to be reduced by thousands of times, so that the actual amount of heat-energy developed in the earth's substance during one year is not large. And we further hold that the greatest part of this heat-energy is, during the year, radiated away into space, only a small residue remaining within the earth's substance. This residue accumulates from year to year and is the cause of the downward increase of the temperature of the earth's substance; for the parts nearer the surface cool sooner than the deeper parts.

Further, the accumulation of this residue continues from year to year, until it reaches a certain limit, when the tension of the interior of the earth's substance exceeds the cohesion and gravitating resistance of the upper strata, and they give way at their weakest points, and the phenomena of an active volcano takes place, thus relieving the tension of the interior, and another reaccumulation of heat-energy is rendered possible.

This cycle of events repeats itself for ever and ever without beginning and without end. The temperature of the interior of the earth practically remains constant-varying only within narrow limits.

PART II.

BOOK V.

EVIDENCE.

BOOK V.

EVIDENCE.

I. We called part I. of this work *Argument,* because in that part of our work we attempted and, let me hope, succeeded fairly well to prove by sheer force of argument,—by mere force of rigid, unsophisticated logic, that is to say, by deduction from positively known laws of nature—certain very important propositions. I allude to propositions IX., X., XI., XII., XIII., XIV., and XV. of Book IV. The essence of all these propositions may be formulated into one grand general law of nature—that **whenever and wherever there is any curvilinear motion whatever produced by the action of any DYNAMIC FORCE WHATEVER on a body in motion, there is, then, molecular energy (heat) continually being developed in that body, as long as the curvilinear motion lasts; and that the amount of heat-energy thus developed is, other things being equal, proportional to three quantities, viz.:—**

(a) The velocity of the moving body.

(b) The intensity of the centripetal force.

(c) The sine of the angle between the direction of the centripetal force and the tangent to the curve in the direction of motion; and that such, also, must be the case, even if the FORCE producing the curvilinear motion be not a definite amount of any form of ACTUAL energy, but even if it be that, much neglected, universal, eternal, and inexhaustible force—GRAVITATION.

It is for the sole purpose of deductively proving this one grand, bold, improbable, nay, even at first sight impossible law that the whole of part I. was written. It is for the sole purpose of making the assertion of this law possible that we so stoutly and boldly fought against that Goliath-giant of a doctrine—*the generalized doctrine of conservation of energy*. It is for the sole purpose of this law that we so boldly and unhesitatingly attacked one by one most of the fundamental principles of static and dynamic mechanics, especially the three laws of motion of Newton and his conception of inertia. Even Newton, the great Newton, the father of modern astronomy; Newton, our reverence and homage to whom knows no bound; even *he* has not escaped our merciless and perhaps reckless criticism. In a word, we have risked all—everything; abandoning ourself as though caring for no consequence, for no one, and for no thing in the world. And all this, for what? For the sole and only purpose of defending and proving this, our grand law.

Now, the candid and considerate reader will understand that one will not sacrifice so much labor and time for the mere highly improfitable purpose of being able to say something, contrary to all and everybody, unless that one be deeply and absolutely convinced of the truth of what he says.

Now, how can one be convinced and convince others of the truth of what he says (aside from pure logic), especially if what he says concerns the nature of things, if not by facts—facts taken from ever truthful and ever constant nature?

In many things, when one asserts something, he can and does prove the truth of his assertions by experiments. But in many other things, when no experiments are possible or practicable, the next best possible proof is to bring forward facts ready taken from nature. And if those facts be numerous, and if they be most satisfactorily explained to their minutae by what he says, and, moreover, if those facts be such that no other satisfactory explanation has ever been given to—then, I say that this mode of proof is positive and decisive and equals in value, if it does not indeed excel, the proof by experiment.

The last is our case. We, therefore, intend in this second part of our work to bring forward numerous, indeed, very numerous facts from nature. We further intend to show that our grand law at once throws a dazzling flood of light on them all; and that this light penetrates these facts through and through up to their darkest and minutest recesses; and that without this light they become enveloped in the profoundest darkness of mystery. And this is why we term this second part of our work EVIDENCE.

We will divide the "evidence" into the following sections:—

I. The Earth.
II. The Planets.
III. The Comets.
IV. The Meteors.
V. The Sun.
VI. The Stars.
VII. The Universe as a Whole.

2. As we would have to recur frequently to the old-timed nebular-hypothesis of Kant and Laplace—recur to it, indeed, in order to show its inability to explain certain things, the present pretended explanation of which is mainly based on it—because of this, we have thought it advisable to say a few words about it in this introduction.

Briefly stated, the nebular hypothesis of Laplace is this:—

Once, a very, very long time ago, the matter constituting now the solar system: the sun, the planets with their satellites, the comets, and the meteors—this matter was once, so Laplace supposes, in a diffused gaseous state and formed a large huge nebula extending beyond the orbit of Neptune. This nebula was spheroid in shape. It was *hot* and slowly rotating from west to east around an axis perpendicular to the present ecliptic, or nearly so.

Now, this nebula, on account of cooling by radiation into space, began to contract (by gravitation?) in its exterior, while its interior became compressed, and this contraction gave rise to an increase of temperature (not heat).

This contraction and reduction of size of the nebula caused an increase in the speed of rotation, according to a very well established law: the law of conservation of areas. This increase of the velocity of rotation, in its turn, caused an increase in the centrifugal force, especially at the equator. Eventually, the centrifugal force became so great as to *overbalance* the gravitating force at the equator of the nebula, and, in consequence of this, on further contraction of the nebula, an equatorial ring of nebulous matter was left behind and did not follow the contracting parent nebula. This ring continued for a while to rotate unbroken; but, eventually, the ring broke into fragments, and the fragments coalesced and formed one spheroidal mass. Thus a planet (Neptune?) was formed, which planet continued to revolve around the axis of the parent nebula, from west to east; that is to say, in the direction in which the ring was rotating.

Further contraction of the parent nebula and further separation of rings gave rise to the formation of the planets one after another: Uranus, Jupiter, Saturn, etc., and also to the comets.

Each of these newly formed planets revolved in its orbit, and at the same time it rotated around a central axis of its own substance, from west to east. Contraction of these nebulous planets, also, increased the rate of their several rotations, and, in consequence, planetary rings were formed, similar to the *nebular* rings out of which the planets themselves were formed. And out of these planetary rings the satellites of the various planets were formed.

This is the nebular hypothesis as maintained by Laplace. Kants' hypothesis differs from it in two particulars.

(a) Laplace's hypothesis concerns itself with the cosmogony of the solar system only, leaving the starry heavens, those multitudes of sun-systems, out of all consideration; and the necessary inference is that each and every sun-system of the starry heavens must have evolved from a separate nebula; thus, the number of nebulae becomes, like the stars, countless.

Kants' hypothesis, on the other hand, derives the whole universe, that is, all the multitudes of sun-systems, including our solar system, from *one* great nebula. And, therefore, instead of the hypothetical "rings" of Laplace, he must assume "zones" to have been separated from the parent nebula; for, otherwise, the stars ought to be all in one plane only.

(b) Laplace's nebula is a *hot* nebula; it is also a slowly rotating nebula.

Kants' nebula, on the other hand, is a cold nebula completely at rest; the necessary rotation of the nebula resulting (perhaps?) from differences of density in different parts of the nebula, or from irregularity of form in the nebula; the heat of the nebula, also, resulting from the subsequent contraction.

(My knowledge of the nebular hypothesis is derived mainly from *Stallo's Modern Physics*, Ch. XV., and from Prof. Darwin's *The Tides,* Ch. XIX., to which I earnestly refer the reader.)

3. The nebular hypothesis, it is thought, explains to satisfaction mainly two things: (a) The similarity of motion and of position of the planets and satellites (and comets?), namely: their common direction of revolution from west to east; their common direction of rotation from west to east; the nearly coincident position of the planes of their several orbits; the decreasing velocities of revolution of the various planets as we advance from the sun toward the orbit of Neptune; and, finally, also, to some extent, the difference of their sizes or masses, and the difference of their respective densities.

(b) The hot, fluid, molten condition of the planets, especially of the earth, in which condition we must, for certain reasons, necessarily assume the planets to have existed at some remote period in the history of the solar system.

4 With due deference to the honor of the great men who have promulgated this hypothesis and of those who up to the present adhere to it, I will allow myself to enter into a critical examination of it.

Firstly, then, the hypothesis *assumes* the *existence* of a primordial nebula; and *existence* and *primordial* imply duration; that is, duration before the process of evolution commenced. Existence for how long? I ask. Necessarily, existence from eternity. There can be no other answer to this question, exclusive of creation of matter out of nothing: a supposition which the originators of, and most of the adherents to, this hypothesis are not willing to admit. If so, then the commencement of condensation (through gravity?)

of the nebula has not been accounted for at all in this hypothesis.

Secondly, the indefinite existence of a cold (non-rotating nebula (Kant's), simultaneously with the force of gravitation, involves a contradiction—for the effect of gravitation is the bringing of particles of matter together, contrary to any imagined nebulosity. Any nebulosity of matter, therefore, presupposes the existence of a force whose effects are contrary to those of gravitation, which force Kant does not assume.

Thirdly, Laplace does assume the existence of such a force, namely: heat. His nebula is hot. Now, since the nebula existed from eternity, the heat must also have existed from eternity, a supposition which can hardly be accepted. For, heat is not a primary and necessary attribute of matter, like gravitation, extension and impenetrability. Any existence of heat, therefore, as far as our experience goes, presupposes a cause for its production and another cause for its continuance in a body surrounded by free space. And neither the one cause nor the other is made apparent in the hypothesis of Laplace. The existence of heat from eternity in any nebula is, to say the least, a conception against which the human mind revolts, and is out of all parallelism with what we know from experience.

Fourthly, Laplace asserts that because the heat of his nebula is gradually radiated away into space, that is to say, because the force counteracting gravitation gradually disappears, while gravitation abides—because of this, a gradual contraction of the nebula ensues, through gravitation. (I cannot see any other cause than gravitation for the contraction of such a nebula; for the force of cohesion is evidently out of question here.) He further asserts that because of this contraction, the velocity of rotation is increased, and this increases the centrifugal force; and that, therefore, eventually, when the centrifugal force at the equator overbalances gravitation, an equatorial ring is left behind.

Now, it seems to me that for the centrifugal force to outstrip, so to speak, the force of gravitation is, under such conditions, an utter impossibility. Were the contraction of the nebula caused by any other force than gravitation—by cohesion, for instance—then might the centrifugal force indeed outstrip gravitation; but not in such a gaseous highly attenuated nebula as this, where the only forces concerned are gravitation and heat.

Take a particle of matter at the equator of the rotating nebula. It is subjected to three forces:

(a) Gravitation, tending to draw it to the centre of the nebula.

(b) Heat, counteracting in some measure this tendency of gravitation.

(c) The velocity of the revolution of the particle, or rather, the centrifugal force due to this velocity.

Now, these three forces must balance each other. And since the revolving particle possesses some centrifugal force, there must be some centripetal force with which this centrifugal force is in equilibrium; that is to say, that the heat does not completely counteract gravitation. There is, then, gravitation of some intensity in this rotating nebula.

Now, let the nebula cool and what will be the result? Clearly this. The intensity of gravitation will be increased and the centrifugal force of the particle will, therefore, become smaller than the centripetal force; and as a consequence of this the particle will be drawn nearer to the centre. But how much nearer? Clearly, neither less nor more than the centrifugal force, which becomes larger and larger while the particle is approaching nearer to the centre, will in the limit allow. I mean, it will approach the centre up to a point where and when the centripetal and the centrifugal force will again be in equilibrium. At this point the further approach of the particle or, more generally, the contraction of the nebula, will cease, for the simple reason that at this point gravitation, being equilibrated by the centrifugal force, will cease to act. On further cooling of the nebula, however, the same sequence of events will, necessarily, be repeated, and so on and on. And, threfore, I fail to see the possibility of any particle or of any rings being left behind.

In our daily experience with heat and cold we are wont to say that heat expands a body and cold contracts it. But while it is perfectly true that heat expands a body, the expression *cold contracts it* is not literally correct; for, *cold* is not a force; it only signifies the absence of a force—heat; therefore, *cold* can not be an efficient cause of any effect at all. In reality it is not the *cold* that contracts, but it is some other positive force whose tendency is to contract and which has been counteracted by heat, and which, further, when the heat counteracting it disappears and the body grows *cold,* assumes its normal action, and contracts the body.

Such a contracting force, in our daily experience, is generally one of the following:

(a) Cohesive attraction. This is the most general and most conspicuous contracting force that we are acquainted with.

(b) All kinds of pressure from without on the body.

(c) The gravitation of an external body on the cooling body; say, for instance, the action of terrestial gravity on a cooling gas.

(d) The mutual gravitation of the particles of the cooling body itself.

Now, in Laplace's nebula, the first three causes of contraction of the cooling body enumerated above are positively to be eliminated, as far at least, as an equatorial superficial ring is concerned. The only cause remaining is the fourth. And if so, we have conclusively shown above that no separation of rings are possible under such conditions.

I confess that it was with great hesitation and almost with reluctance that I wrote this fourth criticism of the hypothesis. For I could not very easily persuade myself that Laplace, the great mathematician Laplace, should propound any hypothesis containing such a mathematical impossibility as this; neither could I believe that the many great mathematicians who have examined the hypothesis should have so utterly failed to detect such a serious defect in this hypothesis; for, it is evident that, if our reasoning be correct, this fourth criticism deals the most deathly and severest blow to the hyothesis by striking at its root, so to say.

But, as on close examination I could not detect any fallacy in my reasoning, therefore, I decided to write what seems to me to be true and leave the rest to the reader to decide upon.

One word more. The only possible defense for the nebular hypothesis is the assumption that theree *was* in the nebula a cohesive attraction; but such an assumption is, to say the least, ungrounded and is absolutely contrary to what we know of the nature of highly attenuated gases, such as the nebula is supposed to have been constituted of.

Fifthly, I say that the nebular hypothesis fails in its purpose of explaining the first of the two facts mentioned above: the similarity of motion and of position of the bodies composing the solar system. For it takes no account of a large number of bodies belonging to the solar system, which bodies, if the hypothesis be true, must also have evolved in a way similar to the evolution of the planets. I refer to the comets. It is very well known that the motion of revolution of some of the comets (a good many) is retrograde—that is, from east to west—contrary to the direction of the supposed rotation of the nebula. How can the nebular hypothesis explain this?

The inclination of the orbits of some of the comets to the ecliptic is also very considerable. Even within the planetary system we find such anomalies (if an anomaly it be) in the satellites of Uranus and Neptune; the inclination of whose orbits is considerable and whose motions are retrograde.

Another fact strongly militating against the nebular hypothesis was pointed out by M. Babinet. It is this: From the period of rotation of the sun (25.3 days) M. Babinet was able to caluculate what would be the period of rotation of the sun were it, the sun, expanded (by heat, for instance), to the size of a sphere whose diameter should equal that of the earth's orbit. His calculation shows that the sun, were it so expanded, could not have rotated faster than once in 3181 years. That means that if the nebular hypothesis be true the earth's velocity of revolution ought to have been 3181 times less than the real velocity.

(See Modern Physics by Stallo. Ch. XV.)

Sixthly, the nebular hypothesis is, I say, entirely inadequate as an explanation of the second of the two facts it was intended to explain, namely, the heated condition of the planets, especially the earth, at some remote period of the history of the solar system.

Let us examine the point closely. Kant's hypothesis assumes a cold stationary nebula, which somehow began to rotate; and the cause of the heat he assumes to be in the subsequent contraction of the nebula. The question at once arises how can contraction cause heat? and at the expense of what energy is this heat produced? There can be no other answer to this question but the following: When the nebula is expanded, each and every particle of matter is at a certain distance from the centre of gravity of the nebula; and this distance represents a certain amount of potential energy of the particle. When the nebula has contracted to any degree whatever, the distance of each and every particl of matter from the centre of gravity has diminished; that is to say, that a part of the potential energy of each and every particle has disappeared; and it is at the expense of this potential energy that the heat is produced. There is no other way for accounting for this heat. For, in this cold nebula there is nothing else but gravitation and distance, that is, potential energy (as it is called by physicists) that may account for the generation of heat. There is, indeed, in this nebula the kinetic energy of rotation; but this kinetic energy of rotation cannot be the source of the heat; for, by the hypothesis, this kinetic energy of rotation is increased and not diminished when the nebula has contracted.

Now, I say, were the nebula at rest and not rotating, then would the supposition, that the diminution of potential energy caused by the contraction of the nebula gives rise to heat, bear the stamp of truth, or at least, a semblance of it. But with a rotating nebula such a supposition is absolutely inadmissable; for, with such a nebula, the *whole* of the potential energy, diminished by the hypothetical contraction, is *wholly* converted into the kinetic energy of rotation, acording to the law of conservation of areas, of which law the hypothesis itself makes such important use. It is really incomprehensible how it is that the hypothesis employs this potential energy for the double purpose of accounting for the increase of the speed of rotation (of which increase the hypothesis stands so much in need) on one hand, and also for the generation of heat on the other hand? So much for Kant's nebula.

Laplace's nebula does not present the above difficulty; for the heat of the nebula, according to Laplace, is not caused by the contraction; the contraction only causing compression of the nebula, and a consequent increase of temperature, but not any increase in the absolute quantity of heat.

But let me ask the reader, does Laplace's hypothesis *explain* the origin of the planetary heat? When we ask: What is the cause of this heat? what do we mean by this question? and why do we ask it altogether?

No one has ever asked, what is the origin of the *extension* of the planets, or of their impenetrability, or of their gravitating force. Why? Clearly, because extension, impenetrability, and gravitation are primary and inseparable qualities of matter, and wherever and whenever matter exists they must co-exist with it. But tell any intelligent man that the earth's interior is heated, and he will at once inquire after the cause of this heat. Why? Clearly, because heat is not a primary and necessary attribute of matter; it is accidental, and its presence requires a cause; and not only its presence but its continuance requires a cause likewise.

Now, what answer do we expect to get to the question as to the cause of the presence and continuance of heat? Clearly, that such and such are the causes that produce and keep up the heat. For, while asking the question we are positive that there must, necessarily, be such causes; the only thing we inquire is, what they are?

Now, what does the hypothesis of Laplace do? It does not point out to us any cause or causes which produce and keep up the heat in question. No, not at all. But, instead, it *assumes* the existence of the heat at a more remote period of time, in some problematical nebula; that is to say, it shifts the question away for millions and millions of years (the greater the number of millions the better), and then—and then what?—and then leaves us lost in amazement and puzzled at the vastness of the time; so that in our confusion we forget about our question and of its real meaning altogether.

Let the reader decide for himself, whether this can be called an explanation. Were it not better to say at once: the planets are hot or were hot, because they are and were; that is to say, because we do not know for what?

"The earth," says an old legend, "rests upon water; the water upon air; the air upon fire; the fire upon the word of God." That is to say, in order to account for the apparent secure rest of the earth *physically,* the legend shifts the question, first, from the earth to the water, then to the air, then to the fire; and for the rest of the fire it gives no physical cause at all, but refers us to the agency of God. Would it not be better to say at once: "The earth rests upon the word of God?" The simile between this and the nebular hypothesis, as far as the question of heat is concerned, is apparent and need not be dilated upon.

Here I end my discussion of the nebular hypothesis, and let the reader draw his own conclusions.

It will be fit, however, to add the following quotation:

"The accumulation of difficulties presented by the nebular hypothesis has become so great, and is beginning to be so extensively realized as to develop a tendency to modify or supplant it by another hypothesis which, * * * etc." (Stallo's Modern Physics. Ch. XV.)

5. The Nature of Evidence. Suppose a theft to have been committed; and suppose the thief to have left an imprint of a shoe on the sand or snow; and suppose a man to have been apprehended under the suspicion that he was the thief.

The first inquiry to be made for the purpose of determining whether the man is guilty or not is, naturally, whether the man wears shoes or not; if it is proved that the man does not wear shoes, he may at once be discharged; if he does, he will be detained. But this fact alone will, evidently, not be sufficient evidence against him; further corroborative evidence will be required by the judge. Now, suppose that the measure of the shoe and that of the imprint were found to coincide—then, of course, will the evidence become much stronger, and the judge will have stronger reason to condemn him. Suppose, further, that on closer examination it was found that the numerous minute markings of the imprint correspond exactly to the numerous minute markings on the supposed thief's shoe—then, of course, the evidence will be conclusive, and the thief condemned with as much certainty as he would were he actually apprehended in the act of stealing. On the other hand, suppose the two sets of minute markings not to correspond, and the guilt of the prisoner is absolutely disproved, and he may be set at liberty, even if the gross measurements of the shoe and imprint coincide.

This illustration shows us at a glance that when a question of causal connection between two things or phenomena is involved, the evidence *for* a causal relation becomes very strong, and even conclusive, when all minor peculiarities of the supposed cause correspond exactly to all minor peculiarities in the supposed effect.

Let there be, for instance, a question whether certain motions are the cause of certain other phenomena; and suppose that we succeed in proving that whenever and wherever those motions are present, the other phenomena are also present. I say, that although this alone is evidence in favor of a causal relation between the two, still, it is not strong enough to be conclusive. But if we succeed in proving that every change of those motions is accompanied by a corresponding change in those phenomena, then, the evidence becomes conclusive.

In general, the correspondence of minutæ between a supposed cause and a supposed effect is almost conclusive evidence of their causal relation. This kind of evidence, I hope, will be the most conspicuous feature of our *Evidence.*

BOOK V.. EVIDENCE.

SECT. I. THE EARTH.

1. In this section we intend to show that *our law,* if applied to the Earth, at once renders *all* the phenomena connected with the Plutonic energy of the earth—even up to their minutæ—intelligible. Indeed, so intelligible, so clear and transparent do they become by the light of *this truth,* that we cannot help but be convinced that *this truth* is indeed a Truth—a most sacred and sublime Truth. Were all other planets and their satellites, the comets and the stars non-existent—the Earth, alone, (so it seems to me), would be sufficient *evidence* to establish the verity of our law.

2. Most of us are probably familiar with the fact that the interior of the earth is hot, very hot indeed. "This is no scientific dream," says Sir John Hershel, "no theoratical notion, but a fact established by direct evidence up to a certain point, and standing out from plain facts as a matter of unavoidable conclusion, in a hundred ways. We all know that when we go into a cellar out of a summer sun, it feels cool; but when we go into it out of a wintry frost it is warm. The fact is, that a cellar, or a well, or any pit of moderate depth, has always, day and night, summer and winter, the same degree of warmth, the same temperature as it is called: and *that* (temperature) always and everywhere is the same or nearly the same as the average warmth of the climate of the place. * * *

"But when we go deeper, down into mines and coal pits, this one broad and general fact is always observed—everywhere, in all countries, in all latitudes, in all climates, wherever there are mines or deep subterranean caves—the deeper you go, the hotter the earth is found to be. In one and the same mine each particular depth has its own particular degree of heat, which never varies: but the lower always the hotter; and that not by a trifling, but what may be called an astonishingly rapid rate of increase—about a degree of the thermometer (F° ?) additional warmth for every 90 feet additional depth, which is about 58° per mile."

(Familiar Lectures on Scientific Subjects.)

It should be noted that the rate of increase of temperature, as given by Sir John Hershel (1° F. per 90 feet of descent), is far below the average; the average being, as at present known, about 1° F. per 60 feet of descent.

That at some considerable depth the temperature of the earth's substance is very high—some artesian wells and the hot molten lava ejected by volcanoes are certain and positive evidence.

3. This is the physical fact. Now, when we come to inquire after the explanation of this fact; that is to say, after the cause or causes that have in the past produced this internal heat of the earth and that in the present maintain it—when we come to inquire after these causes and begin to consult various authorities on the subject, we are apt to become confused by the many varied and different opinions, or, rather, guesses, that have from time to time been made and expressed by men prominent in physical and astronomical science. Some of those guesses are very in-

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genious, some of them very fanciful; but all of them fail, absolutely fail to convince and satisfy the inquiring mind. So much so, that even the authors themselves of those opinions and guesses seem far from being convinced, for they generally express themselves in a manner indicating uncertainty and doubt.

Some of these so called hypotheses consist merely in a shifting of the question from the present to the past; only to leave us in darkness and uncertainty as to the cause of the heat then—in the remote past. They assert, namely, this: the earth's interior is hot at present because it was very hot and molten throughout at some very remote period, and since then has succeeded in cooling off to the present condition. And, so the propounders of these hypotheses think that they have succeeded in explaining the fact. First they infer that, since the earth's interior is hot, and since the earth is constantly losing heat by conduction and radiation and otherwise, therefore, the earth must have been hotter a year ago, and it must have been very much hotter a million years ago; finally, it must have been so very hot at some still remoter period as to have been in liquid molten condition throughout. This they infer; and, then, they turn around suddenly and employ this inferred fact as an explanation of the present fact. No one, I think, will admit that this mode of reasoning is in accordance with the principles of sound logic; for, when we ask for an explanation of the present fact, we ask for an explanation of it, together with all that is inferable from it.

The nebular hypothesis of Laplace belongs also to this class of hypotheses, with the only difference that, instead of stopping at liquidity, it goes further and infers gasuity.

Other authorities, while they, too, shift the question of the present fact to the past,—the remote part—assign, or, at least, attempt to assign, a cause for the production of heat in the remote past. Poisson, for instance, thinks "that the heat observed in the earth's crust has been stored up while the solar system was passing in long past ages through a warm region of space." (Rodwell's Dictionary of Science, under Heat.) Lord Kelvin thinks that the heat has been produced in the remote past by collisions of meteors moving in different directions with great velocities, through the conglomeration of which the earth was formed.

Now, I say, would those authorities have shifted the question of the present fact to the remote past, as they have done, and then assign to it a cause with which we are familiar—then, would their explanations amount to something at least; but first to shift the question from the present to the past, and, then, assign to it a cause which we can neither affirm nor negate, as a "warm region of space," or the "collision of meteors," and call this an *explanation*—seems to me to be highly illogical.

Still another fact should be noted. These hypotheses assert that the earth is at present cooling from an original high temperature; and this involves the idea of age: I mean that, acording to this idea, the duration of the earth, at least since the supper crust solidified, becomes limited; the earth becomes aged; it must be so and so many years old. And this seems to the propounders of these hypothesis a necessary inference. But, I say, that this inference is not at all a necessity, as will be gathered from what follows.

There is one hypothesis that deserves special mention. It is that of Mallet. Mallet thinks that the earth's central heat is due to its contraction and consequent pressure and friction. This is somewhat similar to the assertion of Kant, which we have discussed in the previous section. The heat is due to contraction; and the contraction is due to what? I ask,—to cooling? If so, cooling causes heat!

I will end this article by the following quotation from Rodwell's Dictionary of Science, under Heat:—

"We have good evidence of an intense source of heat within our globe in the molten lava, which is ejected from volcanoes, but no satisfactory hypothesis has been adduced to show the cause of its central heat."

4. We will now leave all these hypotheses and apply ourselves to our own law as affording an explanation of this mysterious fact of the present; and we at once say that our law has the following two advantages:—

(a) It explains the present fact by present causes.

(b) The cause assigned is not some fitful invented occurrence of the remote past: an *occurrence* which *happened to occur* once upon a long, long time "in the beginning of things," but does not happen any more; an *occurrence* the like of which neither we nor our grand, grand, grand * * * parents have ever witnessed, and the like of which neither we nor our great grand * * * children will ever witness; an *occurrence* not founded upon law, and not within the pale of mathematics, but even an open contradiction of a good and sound law; the law of uniformity of nature —No! such is not the nature of the cause we assign; for it is itself a law, a grand and sublime law of nature; a law, moreover, founded upon logic and to be inferred through simple but sound reasoning from the phenomena of *motion, force,* and *energy,* as they present themselves to us daily here upon the earth, as we have succeeded, I hope to prove in our "Argument."

We say, namely: The earth's interior is hot because the earth in its revolution is (almost) constantly deflected from the straight line of its motion by a force —*gravitation;* and because it is a necessary law of nature, that whenever and wherever a moving body is deflected from its course, by any force whatever, exhaustible or inexhaustible, constant or inconstant, any dynamic force whatever, molecular energy (heat), in measureable quantities, acording to the amount of motion and deflection, must, necessarily, be produced in the deflected body.

And we say, further, that the heat thus developed continually in the earth's substance is promptly radiated away into space from its upper strata; but that, because of the slow conductivity of the earth's substance, it accumulates in its deeper strata up to a certain limit, as we had occasion to hint above.

It will be understood from this, that the real cause of the earth' heat is in the end, so we hold, nothing else but the constant inexhaustible and eternal force *gravitaiton*—namely, the sun's *gravitation.*

5. So much for the gross explanation of the gross fact of the present interior heat of the earth. Now, for the correspondence of minutæ between the cause and effect, which we esteem the strongest possible evidence of causal relations, as was explained in the introduction to this part.

In what follows we will notice three prilncipal correspdonces of minutæ, viz.:—

(a) Constancy of the earth's central temperature within narrow limits.

(b) Volcanic eruptions.

(c) Change of the plutonic activity corresponding to a change of its causes. I mean, corresponding to a change in the velocity of the earth's revolution and a change of intensity of the sun's gravitation; on which factors the quantity of heat developed should depend, as was sufficiently explained in Book IV.

6. AS TO THE FIRST CORREPONDENCE.

Suppose a large ball of any material to be uniformly heated throughout its substance by a constant source of heat, so that in equal times the ball receives equal quantities of heat, and the heat thus received is uniformly distributed throughout its substances; and suppose the ball to be surrounded on all sides by a medium of constantly low temperature.

The temperature of such a ball, under the above specified conditions, will, after the ball has attained a certain temperature, remain constant, no matter how long the heating will be continued. This is a very well known principle of physics and is a direct consequence of Newton's law of cooling bodies, viz.:—

"The quantity of heat lost (or gained) by a body in a second is proportional to the difference between its temperature and that of the surrounding medium." (Gannot's Physics.)

Gannot further states as a necessary consequence of this law that:

"When a body is exposed to a constant source of heat, its temperature does not increase indefinitely, for the quantity which it receives in the same time is always the same; while that which it loses increases with the excess of temperature over that of the surrounding medium. A point is reached at which the quantity of heat emitted is equal to that absorbed, and the temperature, then, remains stationary."

Now, I say, that the earth—if the cause we assign for its central heat be the true cause—is exactly similar in this respect to the hypothetical ball; for the earth, according to our theory, receives, or, rather, gets developode in its substance equal quantities of heat in equal times—if times be measured by years. Therefore, if *our* cause be the true cause, the earth's temperature, or, more exactly, the absolute quantity of its plutonic energy *ought,* on the whole, be continually the same. As for the qualifications "on the whole" and "within narrow limits," this point will be examined and explained later on.

This, then, *ought to be* the condition of the earth's plutonic energy. Now, what is the fact? Does the fact correspond to what *ought to be?* I say most positively and assuredly, yes!! as we are going to prove in the following articles.

7. Physicists say that the earth's plutonic energy *must have been* greater a year ago, still greater ten years ago, still greater a million years ago, still and still until I don't know what. And they say so, not because they actually found the case to *have been* so. No! absolutely no! for they, themselves affirm nearly the contrary as far as the actual fact goes.

The illustrious James Hutton said in 1785 "that in the material from which geological evidence is to be compiled there can be found 'no traces of a beginning.'" (Geike's Geology, Book I.)

No one since that time has ever dared to deny this sublime assertion of Hutton on the ground of *fact.* No! the geologists cannot point out the fact that the earth's heat *was* greater a million years ago; still they persist in asserting it and combating Hutton with his solid facts. Why? Because, is the answer, *it must be so;* because the earth has radiated heat away into space all this million of years; consequently, a million of years ago, before it has radiated away so much heat into space, *it must have been hotter.* This is the only argument with which they, the physicist and geologists, combat Hutton and his facts. This, I say, is the only argument and there is no other.

8. But as soon as you admit that the earth was once a very hot body and is now in the process of cooling, the idea of age at once makes its appearance—the earth becomes "aged," as was pointed out above. And not only does the earth become "aged," but the sun goes with it; for, the very same reasoning that the physicists apply to the case of the earth's heat, is applicable also, and perhaps with more force, to that of the sun's. And here a great difficulty arises;for the very same physicists assure us that the age of the sun cannot exceed thirty million of years, while the age of the earth cannot be less than one hundred millions of years at the least, as has conclusively been proven from the nature of sedimentary rocks, and from the succession of organic remains in these rocks. (See Gieke's Geology on the Earth's Age and also Darvin's Origin of Species on Geological Record.)

The following is a minimum estimate of the earth's age according to Dr. Houghton:—

"Estimating the present rate of deposit of strata at one foot in 8616 years, assuming the former rate to have been *ten times more rapid* or one foot in 861.6 years, and taking the thickness of the stratified rocks of the earth's crust at 177,200 feet, he obtains a minimum of 200,000,000 years for the whole duration of geological time. (Gieke's Geology, Foot Note.)

It comes out then that the earth is "older" than the sun! The child is older than its parent. Physicists are utterly at a loss how to rectify this difficulty.

"A very simple calculation," says Prof. Newcomb, "shows that were there no source of supply, the sun would be cooled off in three of four thousand years. Whence comes this supply? During the past thirty years the source has been sought for in a hypothetical contraction of the sun itself . . . . Granting that this is and has always been the sole source of supply, a simple calculation shows that the sun could scarcely have been giving its present amount of heat for more than twenty or thirty million years . . . . But the geologists tell us that the age of the earth is to be reckoned by hundreds of millions of years. Thus arises a question to which physical science has not been able to give an answer."

9. We thus see, that the idea of the *inconstancy* of the earth's temperature, dragging, as it necessarily does, that of the sun's with it, involves us in a great difficulty, from which we cannot extricate ourselves. This fact alone, even apriorily, bespeaks of a constancy of the earth's central heat and also of that of the sun. So that, in realty, neither the earth nor the sun are neither "young" nor "old;" neither can we properly say of either of them when it was "born," nor when it is going "to die."

It is hardly necessary to add that *our* assigned cause for the earth's central heat, once accepted, rectifies this difficulty at once; for if the earth's heat is constant, the sun's, likewise, must be constant for the same cause, as we hope to be able to prove when we come to deal of the sun. All idea of "age," with reference to the earth and sun, at once vanishes, and the difficulty is rectified! These considerations, therefore, lend great support to our theory.

10. But we need not rely entirely or even mostly upon *considerations* like the above in order to prove the constancy of the earth's central heat. For we have at our disposal a certain *fact,* a most solid and substantial fact, that most emphatically and decisively asserts this constancy. It is this:—

Since volcanic eruptions are, admittedly, dependent on the earth's plutonic energy, which is their cause; therefore, we ought, and must expect, to find the intensity and frequency of these eruptions varying with the amount of plutonic energy the earth possesses or has possessed at any time. For effects *must* be proportional to their causes. Therefore, if the earth's plutonic energy had been greater, a million, or many millions of years ago, we ought to expect to find that the magnitude, intensity and frequency of the volcanic eruptions had also been greater than what it is now. And since the earth is constantly writing her own history in unmistakable characters upon the rocks composing her substance, therefore, if such had been the case, we ought to find this important fact (more than any other fact) recorded in the pages of the earth's history, in large, easily legible characters. So we ought. But do we actually find this so hecorded? No, absolutely no! On the contrary, we find that the behavior of volcanic phenomena and also of all the other phenomena, whose cause is the earth's plutonic energy, has been the same a thousand, a million, many millions of years ago: the same as the behavior of those very phenomena at the present time.

To the reader who doubts this statement we say: Take any modern book on geology and consult, not the author's opinion, but the facts recorded, and convince yourself of the perfect truth of our statement. Every part, every section—nay, even every page of any modern book on geology, from the first page to the last, bristles with evidence of this constancy to the attentive and observant reader; and no amount of quotations will succeed in convincing the reader as much as the procedure advised here.

The following quotation, however, will not be out of place here:

"From a survey of volcanic rocks of different ages," says Prof. John W. Judd, in his admirable work "Volcanoes," "we are led to the interesting and important conclusion that the scene of volcanic action has been continually shifting to fresh areas at different periods of the earth's history. We find repeated proofs that the volcanic energy has made its appearance at a certain part of the earth's crust, has gradually increased in intensity to a maximum, and then as slowly declined. But as these manifestations have died away at one part of the earth's surface, they have gradually made their appearance at another. In every district which has been examined, we find abundant proofs that volcanic energy has been developed at certain periods, has disapppeared during longer or shorter periods, and then reappeared in the same area."

He then goes on to prove this statement by facts, solid and incontestable facts taken from many parts of the earth's crust. He then concludes by saying:

"From these facts geologists have been led to the conclusion which we have already enunciated—namely, that the subterranean forces are in a state of continual flux over the surface of the globe. At one point of the earth's crust these forces *gradually gather* such energy as to rend asunder the superincumbent rock masses and make themselves manifest at the surface in the series of phenomena characteristic of volcanic action. But after a longer or shorter interval of time—an interval which must probably be measured by millions of years—the volcanic forces die out in that area to make their appearance in another.

"Hence, although we may not be able to prove the fact by any mathematical demonstration, a strong presumption is raised in favor of the view that the subterranean energy in the earth's crust is a *constant quantity* and that the only variations which take place are in the locality of its manifestation."

A little further on he goes on to say:—

"When we examine with due care the lavas, tuffs, and other volcanic ejection which constitute such mountain mass as the Hebrides, of the Auvergne and Hungary, we find clear proof that the ancient Miocene volcanoes of these districts were clothed with luxurient forests, through which wild animals roamed in great abundance. The intervals between the ejections of lava streams were often so great that soils were formed on the mountain slope, and streams cut deep ravines in them."

He concludes this subject a little further on by the following remark:—

"These considerations lead the geologists to conclude that the evidence afforded by the ancient volcanic rocks is *clear* and *positive evidence* in support of the view that the manifestations of the subterranean forces of the past agree precisely in their *nature* and their *products* with those taking place around us at the present time. On the question of great secular changes having occurred in the *amount* of volcanic energy of past geological periods, the evidence must be pronounced negative or at least doubtful." (Volcanoes, Chapter IX.)

Need we say more? The facts recorded in the pages of the earth's history raise a strong presumption in favor of the view that "the subterranean energy in the earth's crust is a constant quantity," and, further, the volcanic mountains of the Miocene age, that is to say of millions upon millions of years ago, were found to have been at that remote period, "clothed with luxurient forests through which wild animals roamed in great abundance." Is this fact, I ask, in harmony with the notion that the earth's temperature has been higher a year ago, ten years ago, etc.? You must not forget, dear reader, that since Miocene age a considerable part of the earth's heated *interior* has been actually turned *outisde* through volcanic ejections, and heat thus lost, separate and appart from the constant radiation through conduction to the surface. Now, if you add the heat thus lost by the earth since the Miocene age to the heat now present in the earth's interior, I don't see, neither do I believe any one can clearly see, how could "luxurient forests" grow, or how could "wild animals" roam on an earth as hot as this. Add to this the fact that if the earth must have been so hot in the Miocene age, the sun likewise must have been very, very hot; for it has been calculated that the sun loses every year a quantity of heat sufficient to raise the temperature of the whole mass of the sun by 2.5°F. Now, since there positively elapsed countless millions of years since the Miocene age, therefore, the sun must have been hotter than what it is now by 2.5°F. multiplied by countless millions; the present temperature of the sun vanishes into insignifiacnce when comppared with its temperature in the Miocene age; therefore, the rays of the sun must have been hotter at that time by many thousands of times than what they are now.

Now, place yourself, dear reader, in imagination upon the earth in what is not called the temperature zone, in the Miocene age, with a hot burning ground under your feet and with a scorching hot sun over your head; a sun whose rays are hotter than those of the present by thousands of times . . . . . (Don't flinch, dear reader, and let your imagination not stagger at the exceedingly high heat of the rays of the sun, for out of charitable and some physical considerations let me reduce that heat to hundreds of times, nay, to to tens of times that of the present) and judge for pourself whether you, dear reader, could continue to exist under such conditions; nay, even whether the wildest of animals could exist; nay, whether even the smallest blade of grass could exist under such conditions.

Professor Judd says that the evidence as to a greater amount of plutonic energy in past ages must be pronounced negative; but I boldly and unhesitatingly say that this negativeness of evidence *for* a greater amount is positive evidence *against* a greater amount; that is to say, it is positive evidence *for* a constancy of the amount of plutonic energy of the earth.

11. It will be of interest here to point out to what greatly extravagant "wild and visionary" notions great men were compelled to resort, because of the notion that the earth's plutonic energy was greater and greater in amount, the further we advance backwards in time. Because, according to the last notion, a time must be reached when the earth was so hot that no life in any form could exist upon it—because of this, a question arises: How did the first germ of life originate upon the earth after it had cooled off so much as to become a fit habitat for life? Of course, it may be said that God

created the first germ of life; but this the great scientific men think unscientific. Lord Kelvin, therefore, suggests as a possible solution of the problem, that a meteoric mass clothed with vegetation has, at that remote period, fallen upon the earth, and from this, by evolution, all life upon earth sprang into existence. This meteoric mass Lord Kelvin conceives to have been a fragment of another world, teeming with life, that has been destroyed by an accidental collision with still another world, and so shattered into fragments, of which this one, that has fallen upon the earth, is one.

Lord Kevlin, himself, calls this notion "wild and visionary;" but still, as there must be some solution to this problem, this is about the best that Lord Kelvin could think of. And all this because of the idea of the inconstancy of the earth's temperature!

So much for the first correspondence.

12. As to the second correspondence.

VOLCANIC ERUPTIONS.

"The modus operandi," says Gieke, "whereby the internal heat of the globe mainfests itself in volcanic action, is a problem to which as yet no satisfactory solution has been found. Were this action werely an expression of the intensity of the heat, we might expect to have it manifest itself in a far more powerful manner in former periods . . . . But there is no geological evidence in favor of greater volcanic intensity in ancient than in more recent pepriods; on the contrary . . . . etc. On the other hand, no feature of volcanic action is more conspicuous than its spasmodic fitfulness." (Gieke's Geology, Book III.)

He then goes on to enumerate various more or less ingenious guesses, or call them, if you so please, *hypotheses,* that have been attempted and maintained by various prominent men entitled to a voice in the matter; with none of which, however, he seems to be satisfied; for first he seems inclined to one and then to the other, or partly to one and partly to the other, thereby showing his great confusion in the matter and his perfect conviction that as yet "no satisfactory solution has been found to the problem."

Indeed, the fact of volcanic eruptions; their sudden character; their periodicity; their dying out of one place only to appear in another place; their appearance, after the lapse of great intervals of time, in the former place—all these seem to be an open contradiction to the laws of physics. That a globe, originally molten and fluid throughout, which has been cooled off so much that a shell of solid cooled off matter has formed all over its surface; a shell, moreover, many tens of miles in thickness; a globe whose balance of interior heat is constantly going on diminishing by conduction and radiation—that such a globe should, after its surface has enjoyed perfect stability for any length of time, all of a sudden be rent at any part of its surface by this constantly diminishing interior heat, without any other cause; and thorugh the rent thus formed large quantities of matter from the heated interior should be forced out; and after equilibrium was again reestablished and rest enjoyed for a length of time, the very same process should again be repeated, perhaps, with greater intensity and on a larger scale than before . . . . . . I say that this is, indeed, contrary to the well known laws of physics. For, in accordance with those laws, once stability of surface has been attained in such a globe, this stability ought uninterruptedly go on increasing with the progress of time and diminution of internal heat, if this internal heat be conceived as the only cause of disturbance. Therefore it is that physicists were compelled to go in search for other additional causes of volcanic eruptions. But this search seems to have been far from successful; for they failed, absolutely failed to find any other cause satisfactorily accounting for these phenomena. And failed, not because of a lack of preservance, skill, or ingenuity; no, not because of this, but I believe, merely because they started with wrong premises: That the earth's heat was *constantly* going on *diminishing;* and, therefore, the whole difficulty.

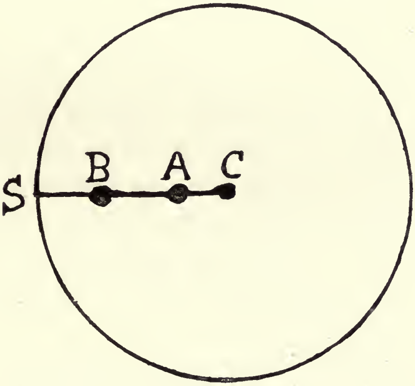
13. Now, I say, once is *our theory* accepted and all difficulty at once vanishes; law and order are seen once more to reign supreme in nature—no mutiny, no contradiction, and no mystery.

For, according to our theory, the globe of the earth is comparable to a large globe, which is constantly heated uniformly throughout its substance by, and from, a constant source; and in such a globe, phenomena similar to the volcanic phenomena on earth must, necessarily, and in strict accordance with the laws of nature, take place.

For, Gannot's statement quoted above (Article 5) that under such conditions a time and temperature of the surface must be reached when the quantity of heat radiated away from the surface equals that received from the constant source, and that, therefore, from this time on, the absolute quantity of heat in the globe remains constant and the temperature stationary throughout the mass—this statement, we say, is not perfectly correct for any globe of considerable dimensions. For since the globe is, by supposition, uniformly heated throughout—that is to say, that the matter near the center receives as much heat, say, in a second, as the matter near the surface—therefore, for the surface to radiate away *all* the heat received, it is necessary that the heat received near the center, at any distance from the surface, and the heat received in a second near the surface should both reach the surface in the same interval of time (in a second); and this implies a greater speed of conduction for the heat received near the centre. In a word, Gannot's statement requires that the heat received by the matter of any part of the interior of the globe, at any distance from the surface, should possess a speed of conduction proportional to that distance.

Let the circle in the figure below represent a section at the centre (C) of such a globe. Draw the radius CS; and we say that Gannot's statement requires, that the heat received by the particle of matter marked A should reach the surface in the same interval of time as the heat received by the particle of matter B; or that the speed of conduction of the heat received by particle

FIG 15.



A should be to that of the heat received by particle B as AS is to BS; which is absurd.

14. Now, imagine the globe under consideration to be at some time of uniform temperature throughout, and the temperature to be such that the quantity of heat radiated at this particular time equals, or rather, would equal the amount of heat received.* Now, since the heat of particle A and of all behind it cannot reach the surface at the same time that the heat of particle B reaches the surface, therefore, it follows that the quantity of heat radiated away from the surface will not be fuly resupplied to the surface by the heat received by all the particles of matter extending from the surface to the center, if the globe be of considerable size. What, then, will occur? A little reflection will convince us that the temperature of the surface will, as time advances, fall and become lower than that required for radiating away the whole heat received; (this last temperature of surface I will call the *critical* temperature); and, as a consequence of this, the amount of radiated heat will become less than that received by the globe, and heat will, therefore, accumulate within the central parts of the globe. Further, the reduction of temperature at the surface will travel inwards as a cold wave, so to speak, with a constantly decreasing intensity; so that at the end of a given time, it will make itself felt at a certain given distance; beyond that distance the temperature will be nearly uniform throughout up to the centre.

Again, the reduction of surface temperature will go on increasing for a certain time; but, then, it will cease, for two reasons: first, because with the reduction the surface radiation becomes diminished; secondly, and principally, because all the time that the reduction of surface temperature is going on, there is, as we have just said, an accumplation of heat within the parts near to the centre of the globe, and a consequent increase of temperature in those parts. This increased temperatume is bound by the laws of physics to travel outward, where the temperature is less, as a heat wave, so to speak; and at the end of a certain time the edge or foot of this wave will reach the surface and will check all further reduction of surface temperature, so that the surface temperature will remain stationary for some small interval of time.

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* This is only for the sake of illustration, for in reality, the central temperature can never equal the surface temperature, but must be higher than it. What is really meant is a minimum central temperature with a critical surface temperature.

But with the further progress of the heat wave, the surface temperature will gradually go on increasing until it will assume the *critical* temperature again. But the process will not stop here; for, since during all the time that the surface temperature was below the critical temperature, the globe was losing less heat than it received, therefore, it is evident that when the surface assumes the critical temperature again, the absolute quantity of heat possessed by the globe must be more than the absolute quantity of heat it possessed at the start. And since in both cases the surface temperature is the critical temperature, therefore, we infer that, when the surface assumes the critical temperature the second time, the internal temperature of the globe must be very much higher than the critical temperature; that is to say, the heat wave must not have spent itself yet. The temperature of the surface will, therefore, go on increasing and will continue to increase up to the time when the summit of that wave, figuratively speaking, will have nearly, but not fully reached the surface. But, then, a backward progress of the surface temperature is bound to take place, because, with the increase of the surface temperature above the critical, the amount of heat radiated becomes more than that recived. The surface temperature will, therefore, in the course of time, begin to fall and will eventually reach the critical temperature back again.

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* See foot note on Page 252.

But this time the absolute quantity of heat in the globe will be the same as at the start, and will be, moreover, nearly uniformly distributed throughout the mass.* For at this point, the amount of heat, *received* by the globe more than what it lost while the surface temperature was *below* the critical temperature, will equal the amount of heat *lost* by the globe more than what it received while the surface temperature was above the critical temperature. Therefore, will the absolute amount of heat be the same as at the start. It will also be distributed through the mass exactly in the same way as it was at the start; for, if the central heat be supposed to be more or less than what it was at the start, then, would the absolute amount of heat not be the same as at the start.

The globe will ,therefore, be in absolutely the same condition as at the start, as far as its amount of heat and its distribution is concerned. The cycle of events described will, therefore, recommence, and will continue to repeat itself as long as the source of heat will last; and, if the source of heat be inexhaustible, will continue to repeat itself for ever and ever.

15. In describing the course of events, in the preceding article, which will, necessarily, and in strict accordance with the laws of physics, take place in the globe under consideration, we consciously and purposely omitted to take proper cofnizance of two very

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* See foot note on Page 252.

important properties of high temperature, or, rather of highly heated matter. The first of these is the fact that a high temperature antagonizes cohesive resistance (or force), liquifying a solid and, if the temperature be further increased, converting the liquid into a gas. The second is the expansive or elastic force of highly heated matter. These two properties of high temperature will greatly interfere with the regular progression of the cycle of events described above; and, therefore, will the cycle of events be completely realized only when the range of the temperature of the globe (when it is at its maximum), is comparatively low, that is to say, when the heat supplied per unit of time per unit of mass is small, and the globe is of comparatively small dimensions; for, with a larger and larger globe the same heat supply per unit of mass and time will produce a higher and higher central temperature, other things being equal, so that a heat supply per unit of time and mass, which in a small globe will produce a very low temperature, , will, if the globe be very large, produce such a high central temperature as to liquify the central part of the globe, or even to convert it into gas.

In passing, let us remark, that the degree of high central temperature will, also, very largely depend upon the heat conducting popwer of the substance or substances of which the globe is composed, being preater with substances of low conductivity.

16. Now, let the given globe be very large; the heat supplied per second per unit of mass, moderate; the heat conductivity of the substance or substances of the globe small—What will result?

Let us, as we have done in Article 14, start with the globe when it is of uniform temperature.* Then, of coures, the surface tempperature will begin to fall, and with it the amount of heat radiated away into space will get diminished and heat will begin to accumulate within the central parts of the globe. The conductivity of the substance of the globe being small, the cold wave started at the surface will peretrate the interior of the globe but slowly, and after the lapse of many, many years will have penetrated but to a certain distance from the surface; all beyond that will be of nearly uniform high temperature; and, if the temperature be sufficiently high, will be in an actual or potential molten condition. So that the globe will present a more or less thick shell of solid matter, cooled on the outside by the constant low temperature of the medium, and heated on the inside by a hot spere of molten matter, whose temperature is constantly increasing. Under such conditions the inevitable must sooner or later happen; and the inevitable is that of the two processes, the heating and cooling processes—the former must predominate; and the heat will progress to the surface; and the surface temperature will commence its backfard journey to the critical temperature.

But while the central heat travels outside it will melt more and more of the inner parts of the shell of solid matter—the crust (the reader has probably guessed by this time that by the "very large globe" I refer to the earth)—so that the crust will get thinner, and its cohesive resistance less and less; while the inner temperature will all the time get higher, and the tension of the confined heated matter greater. Eventually, even before the surface tempperature has made much progress towards the critical, the crust will have become so thin and the internal tenison so high that the crust will give way in its weakest parts and a general

* See foot note Page 252.

convulsion of the earth will take place: an earthquake of more or less magnitude or a local volcanic eruption will take place and the interior of the earth will rid itself of much heat and of some considerable quantities of heated and molten matter—lava. The interior tension will then be relieved and the temperature of the interior will fall; the progress of heat-wave will be arrested, and the surface temperature will, perhaps, suffer some retrogression from the critical, and quiettude will reign for some time.

But this condition of affairs cannot last for an indefinite time; for the surface temeprature being all this time less than the critical, a reaccumulation of heat in the interior is inevitable; the subterranean forces will gradually gather such energy as to manifest themselves again in the phenomena in the same pplace as before. As time advances, however, the forces immediately underneath a volcanic vent will gradually diminish, for the simple reason that the loss of heat entailed by the repeated volcanic menifestations will have more direct and immediate effect upon the heated matter underneath the vent. Further, the crust at and in the immediate neighborhood of the vent will gradnally get thicker and thicker, not only from above by the lava poured out, but also from below by the cooling and consolidation of the molten lava underneath, as we have just pointed out; so that in the course of time, the old vent will offer greater cohesive resistance than some other parts of the earth's crust, and the volcanic phenomena will shift to that place.

17. Let us remark right here that the place of least resistance of the crust will be where the substances forming the upper strata have the less heat-conductivity. For, evidently, the liquifying process under-

neath will go on in such places with more vigor and intensity than in all other places; the crust will here, therefore, be the thinnest, and its cohesive resistance, consequently, the smaller.

This consideration, it seems to me, at once explains the otherwise inexplicable fact of the proximity of volcanoes to the sea. For, for considerable distances from the sea, the earth's srust is saturated with salt water, which is the worst heat conductor on earth. For liquids, as is well known are worse conductors than solids; and of liquids, water is the worst; and of water, that holding salt in solution is the *worst.*

"Paazlow states that in regard to conducting power the following liquids stand in order given of their *decreasing* conductivity for heat; mercury, water, sulphuric acid, solution of sulphate of zinc, *solution of common salt.* (Gannot's Physics.)

It might be asked 'If so, volcanic phenomena ought to manifest themselves with more frequency at the bottom of the ocean than on the coast?' My answer is, that the low temperature that must necessarily prevail in the water at the bottom of the ocean, is an element that will tend to diminish the frequency of volcanic manifestations there.

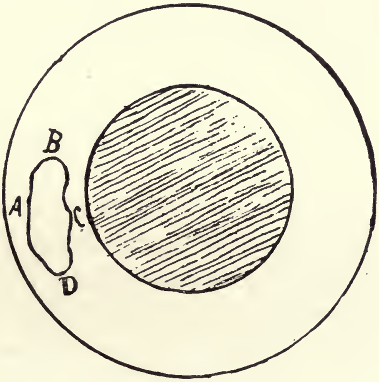
18. At the risk of tiring the patient reader I will add the following two considerations.

The first is that in a globe like the earth, under similar conditions, the upper strata must, necessarily, possess less heat conductivity that the central part. Firstly, because heat conductivity increases with the density of the substances, and the density of substances is evidently greater the deeper we proceed into the interior of the globe. Secondly and principally, because any portion of matter within the earth's interior, possessing *less* conductivity than the rest, will sooner or later be ejected to the surface. For such a portion of matter will evidently contain more heat than the rest, it will, therefore, be *lighter* than the rest; and will, therefore, find its place right underneath the solid crust, ready to be ejected by eruption.

The second consideration is that some volcanic phenomena may have nothing to do with the forces residing in the interior molten sphere.

In the figure below let the space between the two concentric circles reppresent the earth's solid crust, and the central space—the molten sphere. Let the space ABCD represent a mass of matter within the earth's crust of very low conductivity. It is evident that in the course of time so much heat may accumulate within this mass as to melt it and result in volcanic eruption.

FIG. 16



19. Before I conclude the subject of our correspondence, let me say that I feel that I am far, very far, indeed, from having done the subject perfect justice.

Firstly, because the problem of a globe heated by a constant source and radiating heat away into space is such a grand and at the same time difficult problem, especially in the present state of our obscure knowledge with regard to certain properties of heat, that a whole voluminous treatise might be written on it alone. Secondly, because, I frankly confess, I am no geologist, neither do I pretend to be one; for, my whole study of geology has been very superficial, indeed, and only as far as the interests of this work are concerned.

But I hope and am firmly convinced that in the hands of the skilful and able geologists and physicists "our theory" will constitute a torch of light that will at once illuminate all prenomena connected with the igneous elements of our globe up to their minutae; and all darkness and confusion, all mystery and riddlesomeness will at once be banished from this sphere of human inquiry for ever and ever.

20. *AS TO OUR THIRD CORRESPONDENCE.*

The two preceding correspondences are undoubtedly of great value and weight, as pointing to a constant heat source as the cause of the earth's internal temperature; but they have absolutely no bearing at all on the question: What *is* that constant source and what is its nature? For, any constant source whatever, of any nature whatever, will satisfy the demands of the preceding correspondences.

Our third correspondence, on the other hand, not only strengthens the conclusion drawn from the preceding two, but, in addition to this, definitely and most decisively points to *the* real source: not only to *a* source, but to *the* source, as will presently appear. And therefore, I consider the third correspondence the most important of the three.

21. Proposition xiv. of Book iv. states that "the amounts of heat developed per unit of time in a planet revolving around the sun in an elliptical orbit, in different parts of the orbit—are inversely proportional to the cubes of the radii vectors of these parts."

This proposition is, in fact, only a necessary corollary from propositions IX, X XI, XII and XIII.

Now the cause we assign for the earth's central heat, as stated in Art. 3 of this section, is nothing else but a certain necessary law of nature embodied and expressed in propositions IX-XIII of Book IV.

Therefore, if the cause assigned be the real and true cause, the amount of heat developed in every unit of mass of the earth's substance per unit of time ought to vary in accordance with proposition XIV; that is to say, it ought to be the most with the least radius vector of the earth's orbit, and vice versa; that is to say, that the amount of heat developed in the earth's substance for any given time ought to be more when the earth is at or near its perihelion than when it is at or near its aphelion. And as the earth is near its perohelion in the winter months and near its aphelion in the summer months, therefore, acording to proposition XIV., the heat developed within the earth in a day or month during the winter ought to be quantatively more than that developed in the same time during the summer.

And if the quantity of heat energy developed in the earth during the winter months be quantitively greater than that during the summer month—then, all phenomena, whose cause is the earth's pplutonic energy, ought to manifest themselves with greater vigor, intensity and frequency during the winter than during the summer. So they ought. And if we should find the case in reality to be so, would not this alone be the clearest and most positive verification of our XIVth proposition, and, together with it, of all the preceding propositions, IX.-XIII., of which the XIVth proposition is but a corollary?

But, dear reader, I actually rejoice to be able to tell you that such is really the case: that the phenomena, whose cause is the earth's plutonic energy, are really, and as a matter of fact, manifested with more vigor and frequency in the winter months than during the summer months; a circumstance that has puzzled the ingenuity of physcicists, astronomers, geogolists alike and to which, after all is said, they were absolutely unable to give any adequate explanation.

22. There is a certain branch of scientific inquiry called "Seismology." It is an inquiry into the nature, causes, etc., of certain minute tremors to which the ground in nearly all places has been found to be subjected. These tremors are always and invariably accompanied by auditory shocks, ticks, and noises of various descriptions. The tremors and the auditory phenomena accompanying them have been measured and examined into by various great and prominent men, in different parts of the world, through the aid of very delicately constructed instruments, such as the microphone, the horizontal pendulum, etc. The literature on the subject is also quite extensive.

Now, these earth-tremors and the phenomena accompanying them have been most positively ascertained to be caused by minute earthquakes, so-colled "microsisms;" the cause of these tremors is, in other words, positively and without the slightest shadow of a doubt—the earth's plutonic energy. Therefore, if our theory be correct, these minute disturbances ought to occur with more frequency and intensity when the earth is at or near its winter solstice. So they ought; and so, dear reader, they actually are.

"The maximum of disturbance," says Professor G. H. Darvin, referring to these tremors, "occurs near the winter solstice and the minimum near the summer solstice."

(*The Tides. Ch.* VI.)

Professor Darvin does not even as much as attempt to explain this very striking and important fact.

23. Not only does the law enunciated in our proposition XIV. of Book IV. hold good with reference to these minute manifestations alone of the plutonic energy of the earth; but in a general way it holds good even with reference to the gross manifestations—earthquakes and volcanic eruptions. For even these occur more frequently during the winter and spring months than during the summer and autumn months.

"Among the Vesuvian eruptions since the middle of the senevteenth century," says Sir A. Geike, "the number which took place during the winter and spring has been to those that broke out in summer and autumn as 7 to 4. In Japan, also, the greater number of recorded eruptions have taken place during the cold months of the year, February and April."

(*Geology. Book III.*)

True, Sir Geike attempts to explain this fact and attributes it to various causes, as increased atmospheric pressure, increased rainfall and accumulation of snow in the northern hemisphere. But Sir A. Geike himself seems to be far from satisfied with these explanations;

nor are those explanations acceptable to the mind at all, even in the absence of a better explanation.

Professor Judd in his "Volcanoes," says the following:

"Whether the popular idea that the outbursts of Stromboli (a volcano) are regulated by the atmospheric conditions has any foundation is still open to grave doubt. It seems to be certain, however, that during autumn and winter the more violent proxysms of the volcano occur, and that in summer, the action which takes place is far more regular and equable."

Of course, let me add, the law enunciated by us in our fourteenth proposition of Book IV is, as is to be naturally expected, more easily and with more definiteness and clearness to be traced in the minute manifestations of the earth's plutonic energy than in the gross manifestations, where our 14th proposition can have but a comparatively small determining effect: the quantity of energy necessarily concerned in these manifestations being so large.

23. As we are nearly at the end of this section, I will ask the patient reader to reread carefully and with proper attention propositions IX, X, and, especially, proposition XI, with its scholium (Book IV). For this section is really an aposteriori demonstration to that proposition, which proposition is really the basis upon which the general law as stated in Art. (1) of this section is based.

24. Before leaving the earth entirely as affording the evidence we are seeking, it will not be amiss to cast a cursory glance at the earth's constant companion —the moon—with the same object in view.

The moon has from immemorial times been the teacher of the scientific inquirer with regard to many an astronomical question. It was the moon who first taught Newton the grand law of general *gravitation,* which we so extol throughout this work. Were it not for the moon this work could have no existence. So we may, therefore, say that the moon has done for *us* inestimable service.

25. The current opinion is, that the moon is an absolutely dead and physically functionless body; that:—

Firstly, it possesses no atmosphere at all, and, consequently can have no vegetation nor any other form of organic life upon its surface.

Secondly, that it possesses no internal energy in the form of heat, as does our earth. The physicists say that the moon did at *one time,* a long, long time ago, in the beginning of things (if you please) possess a great quantity of this internal heat; but, that since then she has succeeded, on account of her small bulk and comparatively large surface, in cooling off entirely and absolutely, just as the earth will do in several millions of years to come (?).

This second assertion is principally based on apriori reasoning, thus:—Since the moon has always accompanied the earth, and since it is inconceivable that the original temperature of the moon should have been higher than that of the earth, it follows, therefore, that if the earth has succeeded in cooling off, the moon's heat must necessarily by this time have disappeared entirely.

26. The first thing to be noticed of the moon with respect to our inquiry is the great number of volcanic mountains that cover the face of the moon. The num-

ber is overwhelmingly great when compared with the number of volcanoes here on earth.

"Galileo," says Professor Proctor, "was the first to recognize the great number of craters which exist on certain parts of the moon's surface. He compared the craters in the south-western quadrant of the moon to the 'eyes' in a peacock's tail."

(*The Moon,* Chapter IV.)

Now, this fact requires an explanation; and, as far as I know, no satisfactory explanation has as yet ever been given. In a subsequent page of the above mentioned chapter, Professor Proctor says:

"If the cratiform mountains were due to volcanic action, then, either that action was more widespread and intense than on the earth, or else the records of such action on our earth have disappeared."

That they were due to volcanic action, of this there can be no reasonable doubt; and the reader can see for himself, how far from satisfactory this explanation is.

Our theory, I persuade myself, does offer a completely satisfactory explanation of this fact. For, firstly, according to our theory, volcanic activity on the moon, just as on the earth, is not a matter of bygone, ancient history, but is going on at present, just as it has done many years ago. Secondly, we have strong reasons for thinking that the amount of molecular energy developed in the moon's substance per unit of time and mass is vastly greater than in the earth. (See last scholium to Book IV.) Thirdly, on studying over Articles 14-16 of this section carefully, we cannot fail to convince ourselves that in a globe of much smaller dimensions than that of the earth, as the moon is, the volcanic eruptions will take place more frequently, although the magnitude of each may not exceed that of the earth, or may even fall behind. This greater frequency will, therefore, explain the greater number of craters on the moon.

27. As to the question whether any volcanic activity is at present going on on the moon—the concensus of opinion of competent and able selenologists is in the affirmative, as many changes of configuration in some parts of the moon's surface, especially in cratered mountains, have been reported by great selenologists, who are entitled to a fair voice in the matter. The history of the supposed changes that have taken place in the Crater Line first in 1788, reported by Shroeter, and then in 1866 reported by Schmidt and Sechi indepently of each other, is very convincing, although some selenologists try to explain it away as an optical illusion. Many other reported changes are also quite convincing.

"It must be remarked, however, that some of the signs of changes remarked by Shroeter, Grinthuisen, Webb, Bert, and others, seem too marked to be regarded as merely apparent." (Rodwell's Dictionary of Science.)

It must not be forgotten that the detection of any active volcanic changes going on on the moon must be extremely difficult, as Professor Proctor has pointed out.

28. On the whole, our *Evidence* from the moon is rather favorable, but far from being satisfactory. I hope, though, that the near future will clear off all doubt upon the point of present volcanic activity on the moon; and, when this becomes positive, our theory needs no better confirmation and can have none better.

SECTION II. THE PLANETS.

1. Before proceeding with the evidence from the planets, we desire to call attention to the following two principles:—

(a) That the critical temperature of a globe receiving a constant supply of heat (see Art. 14-16 of Sect. I, Book V) will largely depend on the radiating power of the materials forming its surface; so that, if two globes of the same dimension and mass be supposed to receive the same constant supply of heat, but if at the same time the radiating power of the substance composing the surface of one be supposed to be less than that of the other—then, will the critical temperature of the first be absolutely higher than that of the second. For, in order for the surface of the first to lose as much heat as the second (as it, necessarily, must: the supply being the same) its temperature must be higher; for, with the same temperature the amount of heat lost by the first will necessarily be less than that lost by the second, since its radiating power is less.

The difference between the two globes, as far as heat radiated away is concerned, will be one of quality, not of quantity. I mean that the rays of heat radiated away from the first globe will be of what is called higher refrangibility than that of the second, so that it may happen that (although they both receive the same constant quantity of heat), the first should be luminous and the other not. There will also be a difference in the specific gravity of the two globes: that of the first being less than that of the second, as is self evident.

(b) The amount of heat developed in a body moving in any dynamical curve must be greater if that body be rotating around a central axis than if it be not so rotating; and the greater the speed of rotation the greater the heat developed, and the less—the less. For rotation is motion, and motion increases inertial resistance, as was sufficiently explained in Part I; therefore is the amount of resistance to be overcome by the centripetal force greater if the body be rotating than if it be not so rotating, and greater with a greater speed of rotation, and less with a lesser speed. And since the heat developed equals the amount of directional inrtia overcome, therefore, our assertion becomes evident.

We will now proceed with our evidence.

2. The planets are usually divided into two groups; first, the intra asteroid planets, comprising Mercury, Venus, Earth, and Mars; secondly, the extra asteroid planets, comprising Jupiter, Saturn, Uranus, and Neptune.

The most striking difference between the two groups is constituted by their respective masses: The masses of the first being comparatively small, those of the second being very large, as will be seen from the following table, the sun's mass being considered as unity.

Names of Planets.	Masses
1. Mercury	$\frac{1}{5,000,000}$
2. Venus	$\frac{1}{425,000}$
3. Earth	$\frac{1}{326,800}$
4. Mars	$\frac{1}{3,093,500}$

Names of Planets.	Masses
5. Asteroids	
6. Jupiter	$\frac{1}{1048}$
7. Saturn	$\frac{1}{3502}$
8. Uranus	$\frac{1}{22,600}$
9. Neptune	$\frac{1}{19,380}$

(After Newcomb's and Holder's Astronomy.)

Now, since an increase of mass involves, other things being equal, an increase of volume; and since, as will be gathered from Arts. 14-16 of the preceding section, with an increase of volume there necessarily goes an increase of central heat even with a smaller constant supply; and since an increase of temperature, necessarily, involves expansion and rarefaction of matter, and consequent diminution of density—we should, therefore, expect, in a general way, to find (if our theory be correct) the greater density within the first group of planets and the least density within the second group. Now, as a matter of fact, such is the case, as will be seen from the following table:

Names of Planets.	Masses	Densities (Water=1)
1. Mercury	$\frac{1}{5,000,000}$	6.85
2. Venus	$\frac{1}{425,000}$	4.81
3. Earth	$\frac{1}{326,800}$	5.66

Names of Planets.	Masses	Densities (Water=1)
4. Mars	$\frac{1}{3,093,500}$	4.17
5. Asteroids		
6. Jupiter	$\frac{1}{1048}$	1.378
7. Saturn	$\frac{1}{3502}$	0.75
8. Uranus	$\frac{1}{22,600}$	1.28
9. Neptune	$\frac{1}{19,380}$	1.15

(After Newcomb's and Holden's Astronomy.)

It will thus be seen that while in the first group the minimum density is 4.17, the maximum density of the second grop is 1.378.

This fact is, therefore, in conformity with our theory and may be considered as affording strong evidence for it.

3. It might, indeed, be asked: "If so, how is it that the density of Jupiter happens to be the highest of all of the second group of planets? Jupiter being the largest and the nearest to the sun, ought not, according to your law, its density be the least of all of the group? How does it happen, then, that instead that of Jupiter the density of Saturn (whose mass is one-third of the mass of Jupiter, and who is also situated farther away from the sun) is the least?"

To rectify this difficulty we must not forget that although the density of a planet ought *principally* depend on its mass and volume, there are other minor causes which will to some degree affect that density: heat conductivity of the substance is one such cause, for, the less the conductivity, the greater will be the accumulation of central heat, and the less the density. The radiating power of the surface of a planet is another cause, as was pointed out in Art. 1 of this section. Hence the irregularities of densities within the second group of planets.

Another and far better explanation is, that although it is true that increase of mass in a planet must tend to diminish its density, this must have a limit beyond which a further increase of mass will tend to *increase* (instead of diminish) the density. For an increase of mass entails an increase of gravitation in the planet; and this, of course, tends to increase the density. Therefore it is that Jupiter's density is less than that of the sun (whose density is given as 1.444), although, unquestionably, the sun's temperature is considerably higher than that of Jupiter; and, therefore, it is also that the density of Saturn is less than that of Jupiter, although, in reality, Jupiter's temperature may be considerably higher than that of Saturn. Saturn's mass may be conceived, so to speak, as the limit mass, of which either an increase or diminution will cause an increase of density.

4. As to direct evidence from the planets, let us say that if we succeed in proving that thermal activities are going on on them and in them, especially if those activities be spasmodic and fitful—then we have the strongest possible evidence in support of our theory. For only by our theory can such spasmodic or rather periodic activities be satisfactorily explained, as was pointed out in Section I of this book; but on no other supposition.

Now, apriorily and in accordance with what was said in Arts. 14-16 of the first section of this book, we should expect to find those thermal activities to be of comparatively low intensity, in the case of the intra asteroidal planets, because of their comparatively small mass and volume. On the other hand, in the case of Jupiter and Saturn, those thermal activities ought to be very marked and of considerable intensity. The great speed of rotation of Jupiter (9 hours, 55 minutes, 20 seconds) and also that of Saturn (10 hours, 14 minutes, 23.85 seconds) will, as was pointed out in article (1) of this section, greatly enhance these thermal activities. So, then, the thermal activities of Jupiter and Saturn, if our theory be true and correct, ought to be found to be on a very extensive scale. So they ought, and so, dear reader ,they are, as will presently be indicated.

5. Direct observational evidence of thermal activities and consequent igneous changes on and in Mercury, Venus, and Mars (the small planets) is entirely wanting. But this, I say, is not because of their absolute absence, but because of our inability to detect them, —just as is the case with the moon. Mercury and Venus also, besides and in addition to the low intensities of the thermal activities to be expected on them because of their small masses, present insurmountable difficulties for close tlescopic observation, with reference to their physical conditions. Mars does not present these difficulties, and, as a matter of fact, we do sometimes notice some temporary changes in the shading of some parts of its surface, which I should be inclined to attribute to volcanic eruptions as to a cause.

"Occasionally, however," says Professor Young, "for a few hours at a time we see others (meaning other markings on the surface of Mars) of temporary character supposed to be clouds. But these are surprisingly rare as compared with clouds upon the earth." (Elements of Astronomy.)

6. Of the thermal activities of Uranus and Neptune we do not, and we ought not to expect to, find direct observational testimony, because of their comparatively small size, their small orbital velocity, and the smal intensity of solar gravitation acting on them; all of which causes conspire to render any thermal activities on them to be on a comparatively small scale. Add to this their great distance from us and the consequent difficulty of telescopic observation with reference to their physical condition.

7. The only planets where in accordance with our theory we should expect to find signs of manifestation of thermal activities of a quite marked degree, and where, in reality, and as a matter of fact, we do find them—are the two giant planets of the Solar System: Jupiter and Saturn.

The mass of Jupiter is 31.6 timess that of the earth; its bulk 1300 times that of the earth; its surface only 119 times that of the earth; and its diameter more than *ten* times that of the earth. The moment of momentum of rotation of the earth almost vanishes into insignificance when compared with that of Jupiter.

Now, as all these items are factors which, in accordance with our theory and what was said in Arts. 14-16 of the preceding section, tend to increase greatly the amount of heat produced in such a body in a definite time (in consequence of its motion in a dynamical curve) and also the amount stored up in such a body (Arts. 14-16)—therefore, we should expect to find the absolute temperature on Jupiter's surface and its thermal activities vastly surpassing those of the earth, in spite of the fact that Jupiter's orbital motion is slower than that of the earth and in spite also of the fact that the intensity of solar gravitation on Jupiter is much smaller than that on the earth.

Now, what are the actual conditions of affairs on Jupiter? We cannot do better than quote the following:—

"But of late," says Professor Newcomb, "it has been noticed that the physical constitution of Jupiter seems to offer more analogies to that of the sun than that of the earth. Like the sun, he is brighter in the center than near the edges . . . The brightness of the center is probably two or three times greatter than that of the limb. . . . A still more remarkable resemblance to the sun has sometimes been suspected—nothing less, in fact, than that Jupiter shines partly by his own light."

He, then, goes on to discuss the pros and cons of this supposition and finally comes to the conclusion that "on the whole, there is a small probability that the brighter spots of this planet are from time to time self luminous."

He further goes on to say:—

"Again, the interior of Jupiter seems to be the seat of an activity so enormous that we can attribute only to a very high temperature like that of the sun. This is shown by rapid movements always going on in his visible surface, which frequently changes its aspect in a few hours. Such a powerful effect could hardly be produced by the rays of the sun, because, owing to the great distance of the planet, he receives only between one-twenty-fifth and one-thirtieth of the light and heat which we do. It is, therefore, probable that

Jupiter is not yet covered by a solid crust, as our earth is, but that his white-hot interior, whether liquid or gaseous, has nothing to cover it but the dense vapors to which that heat gives rise. In this case the vapors may be self-luminous when they have freshly arisen from the interior, and may rapidly cool off after reaching the upper limit to which they ascend."

(Popular Astronomy.)

Professor Proctor in his work *The Expanse of Heaven,* after conclusively proving that Jupiter's substance must be intensely hot, says:—

"It would, indeed, seem as though the actual globe of Jupiter were red hot; since from time to time when the great, white cloud-belt which surrounds his torrid regions, has been dispersed, a strange fiery hue has been observed over this zone, which strongly suggests the idea of a glowing central globe!!"

Professor Proctor, indeed, goes so far as to allow himself the possibility to look upon Jupiter as a Sun and his satelites as planets peopled with inhabitants; which planets receive a good part of their light and heat from Jupiter. (See the *Expanse of Heavens* in the section entitled *Jupiter's Family of Moons.*)

8. Up to this point we succeeded, I hope, in proving that the temperature of Jupiter's surface is exceedingly high, and that there are constant thermal activities going on on its surface or in its atmosphere due directly to this constantly high temperature of surface. This fact alone is strong evidence in favor of our theory, for at present there is no satisfactory explanation to this, except it be that highly mysterious, highly hypothetical, and highly paradoxical "contrac-

tion" of Helmholtz, which the scientific world has been *compelled* to accept as an expplanation of the sun's constant invariable supply of heat energy. I say "compelled," advisedly, because I find many authorities accept this contraction hypothesis under a protest, so to speak, and merely because if not, the constancy of the sun's temperature remains unaccounted. This point will be considered more fully when we come to treat of the sun.

9. That, besides this constant high temperature and these constant thermal activities, there are taking place on Jupiter thermal activities of a spasmodic and periodic nature—of this there can be no doubt; as evidenced by various dark and bright sspots, which, from time to time, make their appearance on the surface of Jupiter, last for a longer or shorter time (weeks or months, or even years) and then gradually vanish. The cause of these spots cannot be anything else but volcanic eruption.

Now, spasmodic and fitful thermal activities, after a period of quiet, afford, as we have pointed out when treating of the earth, very strong and almost conclusive evidence in favor of our theory.

10. So much for Jupiter. As for Saturn—its physical constitution, so astronomers say, seems to resemble very closely that of Jupiter.

"As to the physical constitution of the planet it is probably much like Jupiter, though it does not seem to be 'boiling' quite so vigorously." (*Elements of Astronomy,* Young.)

The following quotation from *Newcomb's Popular Astronomy* will certainly be of interest.

"On the evening of December 7, 1876, Professor

Hall, who had been engaged in measures of the satellites of Saturn with the great Washington telescope, saw a brilliant white spot near the equator of the planet. It seemed as if an immense eruption of white-hot matter had suddenly burst up from the interior. The spot gradually spread itself out . . . so as to assume the form of a long light streak. . . .

"It continued visible until January when it became faint and ill defined, and the planet was lost in the rays of the sun.

"Immediately upon the discovery of this remarkable phenomenon messages were sent to other observers in various parts of the country, and on the 10th it was seen by several observers, who . . ."

Can we even for a second doubt the meaning of this phenomenon? Is it not as clear as daylight that on the 7th of December, 1876, a tremendous eruption of white-hot lava took place in some volcanic mountain on Saturn? Certainly, yes! There can be no reasonable doubt about this.

10. The eccentricity of the orbit of both Jupiter and Saturn is considerable, being .0482519 for the former, and .0559428 for the latter, according to Newcomb. We should, therefore, expect to find in accordance with Proposition 14 of Book IV, the thermal activities of both planets, especially those of Jupiter, greatly increased either in frequency or in intensity, or in both, when the planet is at or near its perihelion. I accordingly hope, nay, I am convinced, that not many times *twelve* years will pass from now before this will be verified on Jupiter (his periodic time being 11.86 years).

Remark. While writing the above I was thinking it worth while to examine all the recorded observations on the thermal activities of Jupiter (as, for instance, the appearance of spots from which its period of rotation has been determined), with reference to this point; that is to say, whether or not they occurred when Jupiter was at or near its perihelion passage; for surely the thermal activities that are more marked are more likely to have been noticed.

Now, on searching, I discovered that the times of *five* out of six recorded observations, that I could get access to, coincide with, or are very near to, the times of the perihelion passage of Jupiter! Is not this the strongest possible verification of our theory? Five out of six recorded observations, all of them occurring exactly at or very near the perihelion passage of the planet! Surely, these five correspondences are not a mere accidental coincidence, but must be due to *a law;* and that law—the 14th proposition of Book IV.

12. Now, to prove our statement:—

In Newcomb's and Holden's Astronomy we find:—

Firstly, that the longitude of Jupiter's perihelion is nearly 12 degrees.

Secondly, that the longitude of the planet Jupiter, on December 31st of 1849, was nearly 160 degrees. So that at this instant of time Jupiter was *after* its perihelion passage and distant from it by 148 degrees.

Now, as the periodic time of Jupiter is nearly 12 years, therefore, Jupiter will describe in one year an arc of his orbit equal to 30 degrees. Therefore, the 31st of December, 1849, when Jupiter was distant from his perihelion by an arc of 148 degrees, was really 4 years and more than 11 months after its perihelion passage; that is to say, that its perihelion passage must have occurred on January, 1845.

Now, three of the recorded observations I alluded to, I found in Newcomb's popular *Astronomy:*

"No further observations were made," with reference to Jupiter's period of rotation, "until the time of Shroeter, who observed a number of transient spots during 1785 and 1786."

That is to say, they made their appearance exactly about 60 years before January, 1845, and, therefore, as is readily seen, they occurred exactly at or very near the perihelion passage of the planet, since its periodic time is 12 years, nearly.

Again. "In November, 1834, a remarkable spot was observed by Madler of Dopart, which lasted until the following April."

Now, the perihelion passage of Jupiter occurred sometimes during 1833; therefore, Madler observed his spot only a little more than one year after the perihelion passage of Jupiter; and, for all we know, the spot might have existed for some time before it fell to the notice of Madler.

Again. "In 1871 Dr. Lohse, of Bothkamp, observed a spot near Jupiter's equator."

Now, the perihelion passage of Jupiter occurred sometimes in 1869; so that the spot was observed two years *after* the perihelion passage—and this I call quite near the perihelion passage—consider, also, as before, that the spot must have existed for some time unnoticed.

The fourth observation I find in Sir John Hershel's Astronomy:—

"they" (the spots) "vary in situation and number as many as ten having on one occasion (October, 1857) been observed."

Now, in the year 1857, during which the maximum of spots occurred, Jupiter was *at* its perihelion.

The fifth observation I find in *Rodwell's Distionary of Science* under "Jupiter."

"During the winter of 1869-70, these belts"—the belts of Jupiter—"were much studied by astronomers, on account of the striking colors and changes of color they exhibited."

Now, these changes of color were undoubtedly due to the intenser thermal activities going on in and on the planet, and they occurred in the year 1869, the year during which Jupiter passed its perihelion.

13. Of the five thermal observations, then, three occurred at the perihelion, one—one year after the perihelion passage, and one—two years after the perihelion passage.

I find, however, a sixth observation recorded in *Outlines of Astronomy*, which does not very much coincide with the perihelion passage, and which, at first, I did not notice.

"They—the spots—," says Sir John Hershel, "were first noticed by Mr. Dawes in the spring of 1849."

Now, the year 1849 occurred fully 4 years after the perihelion passage of Jupiter. But this should not at all discourage us. For, we are far from saying that spots do not altogether occur but at the perihelion passage. We only say that the most conspicuous or most numerous spots occur at the perihelion; and those of 1849 do not seem to have been in any way remarkable.

However the case may be, it is not to be doubted that these five coincidences of the six observations are very strong evidence in favor of our theory.

Here we will leave the reader to conjecture for himself, and draw his own conclusions as to the value of the "evidence" derived from Jupiter and from Saturn.

SECTION III. METEORS AND SHOOTING STARS.

1. Our inquiry with respect to these bodies is not as to their origin, nor as to their chemical composition; it extends only to the cause or causes of the unquestionably great heat-energy displayed by them, as is manifested by their luminosity and other phenomena.

2. At the outset let us state that the height at which these bodies become *visible to us* has been ascertained, as Sir John Hershel states, to vary from 140 miles from the earth to less than this. I say "visible to us," because, for all we know, they might be luminous at distances much greater than this, but on account of their small dimensions their luminosity does not, in a general way, reach us, if the bodies happen to be at greater distances than 140 miles.

3. The current opinion is that the cause of this heat-energy is the destruction of the kinetic energy due to the great velocity of these bodies relatively to the earth (some 45 miles per second, or less) by the resistance of the earth's atmosphere, which is supposed to extend to such a height (140 miles), although there are certain valid facts which tend to show that the atmosphere does not extend as high as this. In fact, the main argument for the extension of the atmosphere to more than 45 miles is constituted mainly by the phenomena of meteors.

4. Now, we wish to say, that granting that the atmosphere does extend to 140 miles and beyond—the amount of resistance the air, in those upper regions of the atmosphere, offers to a body moving with a ve-

locity of even 45 miles per second, and greater, does not seem sufficient to give rise to the incandescence of that body: the density of the air in those upper regions being so small, and the amount of the resistance air offers to any given motion being, of course, proportional to its density.

5. In order to define our position clearly, let us determine what will be the density of the atmosphere at a distance of, say, 105 miles from the earth when compared with its density at the sea-level?

The folowing rule for determining the density of the atmospher at any given height, I quote from *Rodwell's Dictionary of Science* under *Atmosphere:*—

"As affording an approximately correct view of the subject, the following easily remembered law may be given:—

"At a height of seven miles the density of the atmosphere is reduced to one-fourth the density of the sea-level, and for every increase of height by seven mils, the rarity of the air is similarly quadrupled."

Now, in accordance with this law the density of the air at 105 miles above the sea-level is only $\frac{1}{2}{}_{3}$ of that at the sea-level. Now, 2^{30} equals the enormous figure 1,073,741,824! That is to say, then, that at 105 miles above the sea-level the density of the air is reduced from its density at the sea-level by more than *one billion times!!*

6. Professor Newcomb in his *Popular Astronomy* tells us that Sir William Thomson "found that a thermometer placed in front of a rapidly moving body rose one degree (F?) when the body moved through the air at the rate of 125 feet per second." (Meteors and Shooting Stars.)

He accordingly calculates that were a body to move through the air with the maximum velocity of meteors (44 miles per second) its temperature would rise to "between three and four million degrees;" for the increase of the temperature was found to be proportional to the square of the velocity. Now, meteors do move sometimes with this velocity; therefore, their temperature ought, theoratically, to rise to this enormous degree in *one second;* and this is how he explains the luminosity of meteors.

Now, with all due respect to Professor Newcomb, let me say that he seems to have forgotten, here, in his *Popular Astronomy,* the differerence of density of the air at a height of, say, 100 miles above sea-level from that at the sea-level, in which Sir William Thomson (Lord Kelvin) has experimented; or, if he did not entirely forget, he seems to have been unaware of the magnitude of this difference.

True, this fabulously high temperature of three or four million degrees would, in one second, develop in a body moving with a velocity of 44 miles in the dense air at the sea-level; but in a body moving with the same velocity in the exceedingly rare air of 105 miles above the sea-level the temperature developed in the body in one second ought to be only $\frac{1}{2^{30}}$ parts of three or four million degrees; that is to say, the temperature developed in the body in one second cannot, at a height of 105 miles (if the body be moving with a velocity of 44 miles), be more than $\frac{1}{250}$ part of a degree.

Now, as the time occupied by such a meteor in traversing the air, must of nevessity be less than 200 seconds, it follows that the maximum rise of temperature in the body due to the resistance of the atmosphere cannot exceel one degree.

7. In their astronomy, Newcomb and Holden, indeed, touch on the subject of the rarity of the air at a distance above the sea-level. They say :—

"This potential temperature"—of the meteor striking the atmosphere—"is independent of the density of the medium, being the same in the rarest as in the densest atmosphere. But the actual effect on the body is not so great in a rare as in a dense atmosphere. Everyone knows that he can hold his hand for some time in the air at the temperature of boiling water. The rarer the air, the higher the temperature he would bear without injury. In an atmosphere as rare as ours at the height of 50 miles it is probable that the hand could be held for an indefinite period ,though its temperature should be that of red-hot iron; hence the meteor is not consumed so rapidly as if struck by a dense atmosphere with planetary velocity. In the latter case it would probably disappear like a flash of lightning."

It is very difficult to grasp the meaning expressed in these quoted sentences. It seems to me, though, that were Newcomb and Holden consciously aware of the fact that the entire kinetic energy lost by a meteor of a pound of mass moving with a velocity of 44 miles in the atmosphere at 105 miles above the sea-level,—that this entire kinetic energy lost would only be sufficient to raise the temperature of a pound of water by one degree—were they aware of this fact, they could not say, as they said, "hence the meteor is ont consumed so rapidly," for one calorie surely cannot melt, much less dissipate one pound of matter (no matter what the temperature).

8. Professor Young in his *Elements of Astronmy* is more explicit on this point:—

"and moreover," he says, "this temperature is independent of the density of the air—depending only on the velocity of the meteor. Where the air is dense the total quantity of heat—i. e., the number of calories developed in a given time—is, of course, greater than where the air is rarified; but the temperature of the air itself where it rubs against the surface is the same in either case. During the meteor's flight its surface, therefore, is heated to lively incandescence and melted, and the liquified portions are swept off by the rush of the air condensing as they cool to form a train."

Professor Young refers to the high authority of Sir William Thomson in support of the statement made in the first sentence of this quotation.

Now, as far as I can see, the principle upon which is based the assertion made in this first sentence must be the following:—

Let a volume of air of given density be given, a certain amount of heat, say, ten calories, will raise the temperature of this air by, let us say, ten degrees. Now, if this air be rarified to one-half of its former density, the volume remaining the same, the ten calories will raise the temperature of this rarified air, not by ten degrees, but by twenty degrees; the quantity of matter being only one-half; so that only five calories will be sufficient in order to raise the volume of the rarified air to ten degrees. Similarly, if the air be rarified to one-fifth of its former density, two calories (one-fifth of ten calories) will raise the temperature of the thus rarified air to ten degrees. In general, the number of calories necessary to raise a given volume of air to a certain temperature is proportional to its density.

Now, in the experiments with the rapidly moving thermometer of Professor Thomson, it is to be assumed, that as much heat-energy as has been ldeveloped in the thermometer in consequence of its motion, just as much heat-energy must have been developed in all the particles of air that came in contact with the thermometer; that is to say, along the track of the thermometer (125 feet) there will be a line of heated air particles, and the sum total of their heat-energy will equal that developed in the thermometer.

Now, let the thermometer move with the same velocity, but in rarified air, whose density is only one-fourth of that of the sea-level; that is to say, at an elevation of seven miles above the sea-level—then, of course, will the heat-energy developed in the thermometer be only one-fourth part of the former, and so also will the heat-energy developed in all the particles of air along the 125 feet track of the thermometer also be only one-fourth of the former. But, as far as *temperature* is concerned, the *temperature* of the thermometer will, indeed, be only one-fourth of the former, but the *temperature* of the particles of the air track of 125 feet will be the same as at the sea-level. For the volume of air concerned in both cases is the same, therefore, although in the upper air track only one-fourth the quanity of heat-energy has been developed, this one-fourth part will be sufficient to raise the temperature of the air track to the same degree as in the former case, since the density is also only one-fourth of the former density and the volume is the same.

From this it will be understood that when Professor Young (referring to Professor Thomson) says, "this temperature is independent of the density of the air," he refers to the temperature of the air and not to the tmperature of the meteor. For, the temperature of the meteor will certainly be raised by a fraction of a degree only ($\frac{1}{250}$ degree) or, more correctly, the heat-energy developed in the meteor in one sconld will be equal to $\frac{1}{250} \times 775$ foot pounds, and this amount of heat cannot raise the temperature of the meteor to more than $\frac{1}{250}$ of a degree. But the temperature of the air track—although the absolute quantity of heat-energy developed in it will not exceed $\frac{1}{250} \times 775$ foot pounds—will be enormous; for this small quantity of heat-energy becomes very large when the minute quantity of matter on which it is spent is taken into consideration (the air being so rare).

9. From what has been said it will become evident that the only way to account for the luminosity of meteors at a height of one hundred miles, on general principles of thermodynamics, is to assume that it is due not to the incandescence of the meteors themselves, but to the incandescence of the rarified air surrounding and in immediate contact with the meteor, every portion of air struck by the meteor becoming momentarily and for the briefest imaginable time incandescent, and as the meteor moves with great rapidity, this incandescent light seems to move with it; and, therefore, it seems that the meteor itself is luminous. The meteor's surface itself cannot be thought, as Professor Young would have it, to become incandescent because of its being surrounded by air of such a high temperature; for the temperature of this rarified is not at all high quantitatively, it is only qualitatively so. For, if the total amount of heat-energy of all the particles

along the track of the meteor, 45 miles long, be collected together, it will hardly suffice to render even a single grain of sand incandescent. To borrow a simile from Professor Newcomb and Holden, a finger could be held in this burning air for any time without the slightest inconvenience, as far as heat-sensation is concerned. The light of such air is not a hot light, but an absolutely cold light; and a thermometer placed in this burning air will register a temperature far, far below zero. This light must be conceived as something similar to phosphorescence.

10. It seems to me that the conclusions arrived at here are (if the principles of dynamics and thermo dynamics involved be closely examined) inexorable, and there is no escape from them. Professor Young himself admits that the number of calories of the rarified air is indeed much less than it would be were the air denser. Now it is on the number of calories, which a heated body can yield in a given time, that its melting effects on a solid body depend. Therefore, I say, that it was rather hasty on the part of Professor Young to conclude that this highly heated air of low calorification is capable to bring the surface of the meteor to lively incandescence, and, more, melt it.

11. But, nevertheless, the luminosity of the meteors at any height above the sea-level seems to be fully explainable by the general laws of dynamic and thermo dynamic principles, as we have just said. There is one fact, however, which those principles, if we understand them correctly, fail to explain. It is, namely, this: The so-called shooting stars make their appearance, say, at an average of 75 miles above the sea-level; and after traversing the atmosphere for some

brief duration of time, they vanish at a lower level than that at which they made their appearance.

"It is found," says Professor Young, "that on the average the shooting stars appear at a height of about 74 miles and disappear at an elevation of about 50 miles, etc."

Now, the explanation of their disappearance is generally assumed to be, as it must, that they are consumed by the heat; that is—dissipated. But the amount of heat developed in the shooting star in traversing the atmosphere at an elevation of even 50 miles above the sea-level, if calculated according to the general principle laid down by us here, will be found to fall far, very far below that required to melt and dissipate the shooting star. The maximum amount of energy that might be developed in such a star at 50 miles above the sea-level cannot exceed 1,500 foot-pounds per pound of weight of the star; and, considering the specific heat of the substance of the meteor to be even that of mercury this will correspond to the temperature of 60 degrees F., far below the melting point of any mineral we are acquainted with.

12. To conclude. It seems to me, then, that the explanation offered at present for the origin of the light and heat of meteors and shooting stars at high elevations, is not entirely adequate nor sufficient to explain all the phenomena connected with these bodies. And, therefore, I thought that this subject might offer, at least, some evidence in favor of our theory; for it is evident, that when these bodies, moving with planetory velocities, approach the earth, they are deflected from their course by the very powerful attraction of the earth, and the amount of heat-energy developed according to our theory in consequence of this deflec-

tion must be very considerable, as will be evident on applying formula VIII of the corollary to Proposition XIII, Book IV to these bodies. In this case FV will be equal to 32 × 44 × 5280, nearly; and it may easily be shown that if the thermal equivalent of Myer and Joule be applied to the motions of these bodies, this amount of heat-energy will be equal to 196 calories per second; and, as these bodies are certainly being deflected for a good many seconds before they approach the earth to within 100 miles—the incandescence of the meteors and shooting stars and all other phenomena connected with them—are perfectly explained. There is one element of uncertainty in this calculation, and this is, the thermal equivalent proper to the motion of these bodies, as was explained in the last scholium to the propositions of Book IV. We have also neglected the angle between the direction of the meteor's motion and that of the earth's gravitation which, of necessity, we cannot determine. But in spite of all this, it can readily be seen that, if our theory be correct, all the phenomena connected with meteors and shooting stars find in it a most satisfactory and complete explanation; while, on the other hand, the explanation generaly offered does not only strain the physical principles involved to their utmost, but is, after all, unsatisfactory and incomplete, and this circumstance, we think, lends *some* evidence, at least, in support of our theory. (See also Arts. 15, 16, under Comets.)

SECTION IV. THE COMETS.

1. I don't know of any phenomena in the physical world more imposing, more interesting, and, at the same time, more puzzling and enveloped in mystery than the phenomena displayed by those "wanderers" of the Solar System—Comets—on their approach to their governing centre—the Sun. For centuries have great astronomers and physicists applied themselves to the study of those physical phenomena with a view of deteremining their cause; hundreds of telescopes have been eagerly turned to these comets, in order to observe the physical changes and operations taking place in and around them. These operations have been carefully observed and minutely described—but as to their causes, these have eluded alike the keen eye of the observer and the acute mental gaze of the philosopher: both of them failed, absolutey failed; to discover them. There is, indeed, no lack of hypotheses—but merely hypotheses and no more; add to this that these hypotheses are far from being easily acceptable to the mind. No wonder, then, that Sir John Hershel was so deeply impressed with the mystery surrounding these phenomena. "It is," he says, "a subject rather calculated to show us the extent of our ignorance than to make us vain of our knowledge, and to cause us to exclaim with Hamlet 'There are more things in Heaven and Earth, Horatio, than are dreamt of in our philosophy.'" (Familiar Lectures on Scientific Subjects.)

Sir John Hershel, further, touching on the question of the final causes of comets—I mean their designation, purpose in nature, and utility to mankind—touching on these questions, he says: "But there is a use, and a very important one, of a purely intelectual kind, which they have amply fulfilled; and who shall say that it has not been designed that such should be the case? They have afforded some of the sublimest and most satisfactory verifications of our astronomical theory—they have furnished us with a proof amounting to demonstration of the existence of a repulsive force directed (under, etc.) from the sun, as well as of that great and general attractive force which keeps the planets in their orbits, etc."

A little further on he says: "I believe that Newton's explanation of the motion of comets, so exemplified, was that which stamped his discoveries in the minds of men with the impress of reality beyond all other things."

Let me add, dear reader, that the explanation of the strange physical changes of comets afforded by our theory will, let me hope, stamp our theory with a most indelible impress of truth and reality, and will leave no further room for doubt, as we firmly hope to convince the reader before we are done with this subject.

2. The comets, like the planets, move around the sun as a center; but they differ from the planets in the form of their orbits. While the orbits of the planets are ellipses of small eccentricity and nearly circular, those of the comets are ellipses of very great eccentricity; that is to say, they are very much enlongated, so that at one part of the orbit (the perihelion) a comet is very near to the sun and the velocity of its revolution is very great, while at another part of the orbit (the aphelion) its distance from the sun is very great and the velocity of its revolution is very small. This is the only difference of which we are positive, between the planets and the comets, barring the dif-

ference of position of the orbit with respect to the ecliptic, which difference is not essential and can have no bearing on the problem at hand.

The motions of the comets also are governed by the same gravitational laws which govern the motion of the planets; from which fact it becomes evident, so it seems to me, that the matter constituting a comet must, as far as its relation to the governing center is concerned, be exactly similar to that constituting a planet. Therefore it is that the strange phenomena observable in comets when they approach near the sun, which phenomena at first sight seem peculiar to comets and not to planets, are wholly unaccountable.

"A theory of the physical constitution of comets," says Professor Newcomb, "to be both complete and satisfactory, must be founded on the properties of matter, as made known to us here at the surface of the earth. . . . Now, this has never yet been completely done. Theories without number have been propounded, but they fail to explain some of the phenomena, or explain them in a manner not consistent with the known laws of matter and force." (Popular Astronomy.)

These are the manly utterance of a great man who does not choose to be blinded by a mere collection of words devoid of meaning, which somebody, great or small, has chosen to term "explanation," or "theory," etc.; for such, indeed, are all the so-called explanations at present and in the past given to those mysterious phenomena.

3. Let us see what these phenomena are: "When a comet is first discovered in a telescope," says Sir John Herskel, "it is for the most part seen only as a small, faint, round or oval patch of foggy, or, as it is called, nebulous light, somewhat brighter in the

middle. By degrees it grows larger and brighter, and at the same time more oval and at length begins to throw out a tail, that is to say, a streak of light extending always in a direction from the sun, or in the continuation of a line supposed to be drawn from the place of the sun below the horizon to the head of the comet above it. As time goes on night after night the tail grows longer and brighter, the head or nebulous mass from which the tail seems to spring also increases and within it begins to be seen what is called a "nucleus" or kernel, a sort of rounded misty lump of light, dying off repidly to a haziness called the "coma" or "hair." (Popular Lectures.)

It should be remarked right here that the tail of comets has been proven by telescopic observations to form at the expense of matter ejected from the interior of the comet, in a manner similar to the ejection of lava from the interior of the earth.

Sir John Hershel, speaking of Halley's comet in 1835, says: "On the second of October (the very day of the 1st observed commencement of the tail) the nucleus which had been faint and small was observed suddenly to have become much brighter and to be in the act of throwing out a jet or stream of light from its anterior part, or that turned toward the sun. This ejection after ceasing awhile was resumed, and with much greater apparent violence on the 8th, and continued with occasional intermittences so long as the tail itself continued visible." (Outlines of Astronomy.)

He further states that the matter so ejected from the anterior part of the comet has been observed to curve backward away from the sun; and so, finally, this matter constituted the tail of the comet.

In his *Popular Lectures* he says of the same comet: "The bright smoke of these jets, however, never seemed to be able to get far out toward the sun, but always to be driven back and forced into the tail, as if by the action of a violent wind setting against them—always from the sun—so as to make it clear that this tail is neither more nor less than the accumulation of this sort of luminous vapors darted off in the first instance *toward* the sun as if it were something raised up, and as it were exploded by the sun's heat, out of the kernel, and then immediately and forcibly turned back and repelled from the sun."

4. The problems presented by these phenomena to which a solution is to be sought are the following:—

(a) Why should a comet be, when first discovered, "a patch of foggy nebulous" matter? Why should it invariably differ in this respect from all other matter we are acquainted with here on earth and in the planets? Why should there not exist in nature a body revolving around the sun in a very eccentric cometary orbit not foggy and nebulous? In the introduction to our "evidence" we have conclusively proven that nebulosity and fogginess cannot simultaneously exist with gravitaiton, without there being a cause for it. What is the cause, then?

This question, extended, will embrace also all the nebula discovered in the starry heavens. To say that the nebula existed as nebulæ from eternity or since creation, and that they are in a process of condensation, is evidently far from being a satisfactory "explanation."

(b) Why should a comet grow *larger* with its approach to the sun? Expansion of matter against gravitation and cohesion surely pre-supposes a cause. What is that cause, then?

(c) What is the cause of the luminosity of comets which is surely not that of the light reflected from the sun, as proven by spectroscopic examination and by other considerations to be discussed further on?

(d) What is the cause of the eruptive phenomena taking place in the comet? At the expense of what energy is constituted the molar energy communicated to the erupted matter? I do not see how the physicists adhering to the generalized doctrine of "Conservation of Energy" can extricate themselves out of this difficulty. The matter constituting the tail is eventually condensed and falls back into the main body of the comet, and in its fall heat is certainly developed, which is dissipated into space. And we ask, just like in the case of the tides, what is the source of the heat? The physicists cannot get off so easily here as they have done with the tides. Assuredly, here the source cannot be the moment of momentum of rotation nor the kinetic energy of revolution.

(e) Why should the eruptive phenomena take place in the anterior part of the comet, more than in any other part? By "anterior part" I mean the part of the surface of the comet which faces the direction of the motion of the comet. Thus, in figure 17, let ABCD be the comet; the line AB the line of motion, and the comet moving in the direction from B to A as indicated by the arrow. The anterior part of the comet is the part of the surface at and near A

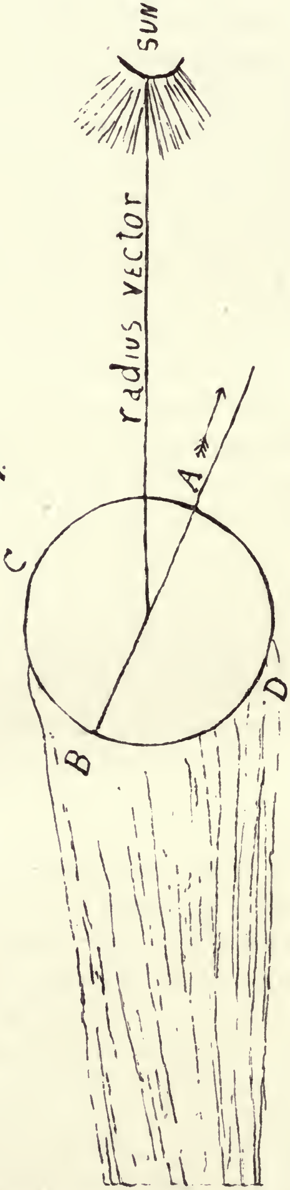


FIG. 17

(f) Why should the erupted matter be repelled from the sun? In other words, why should the tail of comets be directed nearly always away from the sun?

(There are some other problems on which we will touch later on.)

Now, all of these problems, each and every one of them, are, have been, and remain up to the present—a great mystery. For assuredly the theories of a repellent force and of particular kinds of "cometic" matter; of electrical discharges; of materialized actinic rays, etc., etc.—these, I say, are not what can properly be called an explanation or solution. They solve one unknown quantity by another, which is surely not mathematical.

5. Now, I say, that our theory of Force and Energy, of inertia, motional and directional, and especially our theory of the composition of energies or what amounts to the same thing our theory of the economy of energy in the composition of velocities or what, again, amounts to the same thing, the economy of energy in curvilinear, dynamic, or centripetal motion—as explained in Book IV of our "Argument"—this theory (which for myself I would not dare call theory, for with me it is law and certainty) at once, and with one mighty stroke,—as only perfect truth is capable of—sweeps away all darkness, all haziness, and all mystery from the face of comets, whom I may call the great teachers of humanity; for, as it was by them that the great law of the great froce—universal gravitation—was verified, so, I hope, it will be by them that the great truth, that this very same gravitation is the only one great eternal and inexhaustible source of all forms and kinds of energy, will be demonstrated, without leaving any further room for doubt.

6. On earth, let there be a question whether a given body is being heated or not ,and, outside of direct sensation, each one of the following three signs, or symptoms, or phenomena, will suffice to make an answer in the affirmative positive; likewise, the absence of each and every one of them will be taken as conclusive evidence for a negative answer. They are:

(a) *Expansion.* If we should find, for instance, a ball of iron expanding and becoming larger in volume, we would not hesitate to attribute this to heat as a cause; more so, if from a solid it should become liquid; more so, if it should become gaseous.

(b) *Radiation.* If from having been a dark body it should begin to emit dull-red rays of light; if from having emitted dull-red rays it should begin to emit bright red rays; if from bright rays it should change to yellow light; if from yellow light it should change to white and bluish light—then we would not hesitate for a single moment to pronounce this change as due to heat.

(c) *Internal Commotion.* If the ball, from having been in a state of quietude, should begin to exhibit signs of internal commotion, such as the ejection of its inner parts outside; if this state of internal commotion should increase in magnitude and intensity—we would not hesitate to say that the ball is being heated.

Now, should the ball manifest these three signs simultaneously, I mean should there occur an increase of its volume, an increase of its light, and an increase of its internal commotion, all of them together and in proportional degrees—by anything that is sacred to you, dear reader, I ask you, would you dare throw the slightest shadow of doubt on the assertion that the ball is not only hot, but being heated? No! Let me answer for you, positively, no!

7. Now, comets when approaching the sun present these three cardinal heat signs proportionally and at once: they gradually expand and at the same time their luminosity also increases and so also increase stheir internal commotion, until they reach the perihelion, the minimum distance from the sun; after which they gradually contract and at the same time their luminosity (generally) dimnishes, and so also does their internal commotion. Is it not, I ask, as clear as daylight from these facts that while the comets approach the perihelion they are continually being heated; and the rate at which they are heated increases with the diminution of their distance from the sun, and diminishes with the increase of that distance?

But this is in exact accordance with our theory as enunciated in Proposition 14 of Book IV, where, on purely mathematical and theoretical considerations and almost apriorily, we come to the conclusion that "The amount of heat-energy developed in a unit of time in a planet or comet revolving around the sun in an elliptical orbit, in different parts of the orbit—are inversely proportional to the cube of the radii vectors of those parts." (Prop. 14, Book IV.)

In a word, we hold that a comet, like a planet, while revolving in a curve around the sun, has heat-energy continually being developed in its substance in consequence of its being constantly deflected from a straight line; but as the amount of heat-energy varies inversely as the cube of its distance, therefore it is that a comet when nearing the perihelion gets rapidly and greatly heated, as manifested by the three cardinal signs of heat discovered in them at that stage; and, therefore, it is also that after its perihelion passage when its distance from the sun gradualy increases, these heat manifestations gradually subside and finally disappear.

When the comet is at a great distance from the sun, before its perihelion passage, its velocity is small; so also is the intensity of the sun's gravitation comparatively small; so also is the sine of the angle between the direction of its motion and that of the sun's gravitation—small. But it is on these quantities that the amount of heat energy developed in it depends, as is evident from formula X of proposition 13, Book IV, FVsna. Therefore is the amount of heat-energy developed in it in a unit of time very small and is rapidly radiated away into space; and the comet is solid and dark, and, therefore, invisible. But as the comet approaches nearer and nearer the perihelion (F), (V), and (sna), all simultaneously increase. So also increases the amount of heat-energy devolped in it in a unit of time, and the comet begins to expand and to become luminous; and we observe a patch of foggy, nebulous matter. With the further approach of the comet, the thermal manifestation becomes more and more pronounced, until the perhilion is reached; after which, (F), (V), and (sna) diminishing, the amount of heat-energy developed in a unit of time become less and less and the comet cools, and the thermal manifestations finally disappear.

8. Our theory, then, at once gives a perfectly natural, clear, and satisfactory solution to the first four problems enumerated in Art. (4) of this section. Nay, it dose more: it correlates the planets to the comets and the comets to the planets; for the earth, for instance, when nearing its perihelion, also manifests signs of increased thermal activity as do the comets. (See Articles 22, 23, etc., of Sect. I of Book V.) So, also, have we succeeded in proving, when dealing of the planets, that Jupiter manifests its greatest and most

marked thermal activity when at or near the perihelion. Could we but devise means of detecting minor physical changes in the minor planets and those of the major planets, which are at great distances from us (Uranus and Neptune)—I firmly believe the same thing would be observed. In a word, the difference between the comets and the planets, as far as physical thermal changes are concerned, it a difference of degree only, but not of kind.

We thus do away with the assumed special cometary substance, and a special force inherent in the sun different from gravitation, which substance and which force some great men have assumed for the purpose of explaining the physical phenomena of comets.

9. Another unexplained fact, which gets readily explained by our theory, is the fact that all the great comets—those that have shined with the greatest splendor and those that have possessed the longest tails, etc. —are those that have the longest periods of revolution and whose orbit is of great eccentricity; while all or nearly all the so-called telescopic comets have comparatively short periods of revolution, and their orbits are of comparatively small eccentricity. The reason for it, in accordance with our theory, is obvious. The former class of comets must necessarily move very slowly at the aphelion and very rapidly at the perihelion; they must also pass the perihelion at comparatively small distances from the sun; and it is on these two factors, the velocity of the comet (V) and the intensity of solar gravitation (F), that the amount of heat-energy developed in a unit of time depends. Therefore, the former class of comets have exceedingly more heat-energy developed in them when nearing the perihelion than the latter class; and, therefore,

they shine with greater splendor and have more magnificent tails than the latter class.

The comet of 1680, Newton's comet, "the most magnificent ever seen," had such a great period of revolution and the eccentricity of its orbit was so great that it was thought by Newton to move in a parabola. "It passed its perihelion on December 8—and when nearest was only one-sixth part of the sun's diameter from his surface." (Sir John Herskel's Familiar Lectures.) At its perihelion passage it had the exceedingly great velocity of 333 miles per second. No wonder, then, that its splendor was so great.

Halley's comet, which appeared two years after Newton's, has a period of revolution of 76 years; its perihelion passage is at a distance of 55 million miles from the sun; and its velocity at the perihelion must also be quite large. And, therefore, it is that Halley's comet is also a "great comet."

Enke's comet, with a period of 3 years and 4 months, is but a small one, being seldom visible without a telescope. Its nearest approach to the sun is just within the orbit of Mercury. Its velocity at perihelion must be comparatively small. Of course, mass must also be a great factor in determining the brilliancy of a comet, and this comet has a very small mass. (See Art. 16, Sect. I of this Book.)

Bielas's comet of 1826 is a comparatively "small comet." And, accordingly, we find its period some 6 years and 8 months and its perihelion distance about 95 million miles. Its velocity must also be comparatively small.

The comet of 1744, with its magnificent 6 tails, is described as a non-periodic comet—of course, that must mean a comet whose period is very very long and whose orbit is very, very eccentric.

The remarkable comet of 1843, with its very long and brilliant tail, passed its perihelion at the inconsiderable distance of 80,000 miles from the surface of the sun!

The comet of 1556, which so terrified Emperor Charles the Fifth, and which is said to have equalled Jupiter in brilliancy, is a comet of very long period.

The great comet of 1811 has a period of 3,000 years!!

Donati's comet of 1858 with his remarkable egrette tail has a eriod of 1,950 years!

The remarkable comet of 1882 approached the sun to within 300,000 miles of his surface.

In a word, all remarkable comets have periods of long duration, and great velocities at the perihelion. And I consider this fact as affording additional evidence (if there be need of it) in support of our theory.

10. Could we plunge a long thermometer into the substance of a comet and observe the column of mercury rising from hour to hour as the comet nears the perihelion—we could not be more certain that the comet is being heated than we ought to be on the strength of these three cardinal heat signs spontaneously afforded to us by the phenomena of comets. For the thermometer would tell us only one thing: of expansion only, and of nothing else; and it would be possible and easy to invent an hypothesis to account for this expansion on other grounds than heat; while the heat signs spontaenously afforded to us by the phenomena are three in number, and, moreover, they occur simultaneously and in due proportions one to the other—when one increases the other two also increase and vice versa. Therefore is it a matter of great surprise to me, how it happens that the astronomers and physi-

cists have left this terra firma (the heat cause) of the explanation of the phenomena to look for some mysterious power in the rays of the sun and for some special kind of cometary matter and the like, as an explanation of the phenomena. Newton, indeed, attributes most of the phenomena to the heat of the sun's rays; but, as the phenomena begin to manifest themselves at great distances from the sun, this hypothesis had, necessarily, to be rejected, and those that followed him invented separate causes for separate phenomnea. True, the physicist could not acount for the *source* of heat, but would it not have been more logical to leave this question as to the *source* unanswered than to distort and separate the facts? So, while the physicists attribute the expansion and internal commotion of comets to some mysterious or unknown causes, they attribute the luminosity of comets to the sun's light. It is reflected light, they say; in proof of which they adduce the fact that the luminosity increases with the diminution of the distance of the comet from the sun. But the comet's light when subjected to spectroscopic analysis is actually found to be that of an incandescent body and not reflected. The physicists and astronomers are therefore puzzled; but they are still unwilling to give up *their theory* of reflection because of the *fact* revealed by the spectroscope; and they say, there must be something wrong about the spectroscopic revelations; (they rather dare deny and doubt the fact than to abandon their own theories).

"The spectroscope," says Professor Newcomb, "if we interprete its indications in the usual way, tells us that a comet is simply a mass of hydro-carbon vapor, shining by its own light. But there must be something wrong in this interpretation. That the light is reflected sunlight, seems to follow, necessarily, from the increased brilliancy of the comet as it approaches the sun and its disappearance as it passes away." (From an address by Professor Newcomb in *Masterpieces of Science.*)

Again, he says in his *Popular Astronomy,* "as the case now stands, we must regard the spectrum of a comet as something not yet satisfactorily accounted for."

But the language of the spectroscope is always clear and definite, and it is very hard to see the possibility of a misinterpretation. Rather discard any inference, no matter how plausible, than the clear and positive testimony of the spectroscope. Further, the expansion and internal commotion of the comet, like its light, also increase with the approach of the comet to the perihelion and diminish with its recession from it; and if the light must on this account necessarily be due to the rays of the sun, so must the expansion and the internal commotion also be due to the same cause, which Professor Newcomb, himself, is not willing to admit. And if these are not due to the sun's rays, the light of the comet, which increases and diminishes with them, must also not be due to this cause; for, it is evident that the three must have one and the same cause.

If any further evidence should at all be considered desirable to prove the incandescent character of the light of the comet, we can adduce the following:—

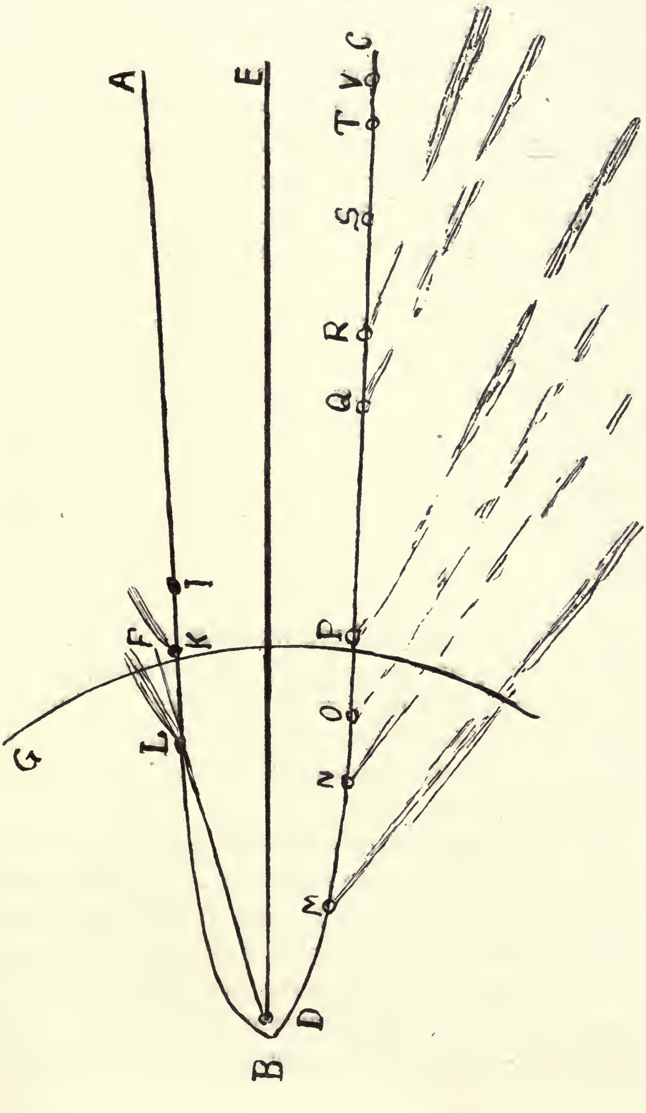
If the light of the comet be reflected from the sun, then, the distance from the perihelion at which it becomes visible, and that at which it becomes invisible after its perihelion passage, should be equal. On the other hand, if the light of the comet be due to a gradual heating of its substance, then, ought the distance at which it becomes invisible greatly exceed that at which it became visible, supposing that at two corresponding points on both sides of the perihelion the comet receives the same amount of heat. For the heat accumulated until the comet reaches the perihelion will require time to be radiate daway, and this will, of course, prolong the period of its visibility after perihelion passage.

Now, the fact is, that the time and the distance of the comet's visibility, after perihelion passage, greatly exceed the time and distance of its visibility *before* perihelion passage. Halley's comet, whose first appearance in 1835 was so eagerly and closely watched for by astronomers all the world over, first became visible on August 5, 1835, and passed perihelion on November 16, 1835; so that its period of visibility before perihelion extended for three months and eleven days. It became invisible after its perihelion passage on the 17th of May, 1836; so that its period of visibility after perihelion lasted for fully six months, almost double that of its visibility before perihelion. I do not see how this can be accounted for on the theory of reflection.

Newton's comet of 1680 first became visible on November 3d (or 4th), without a tail; it passed the perihelion on December 8th, that is to say, some 35 days since its first appearance. It became invisible on March 9, 1681, that is to say, some three months after perihelion; so that, its period of visibility after perihelion was almost three times as long as its period of visibility before perihelion.

The annexed figure is copied from Newton's *Principia,* and represents the orbit of the comet of 1680 and the different places of it in the orbit in different dates, and also the different comparative magnitudes of the tail in those places. "ABC represents the trajectory of the comet, D the sun, DE the axis of the trajectory, DF the line of nodes, GH the intersection of the sphere of the *orbus magnus* with the plane of the trajectory, I the place of the comet on November

FIG. 18



4th, Ann. 1680; K the place of the same on November 11th; L the place of the same on November 19th; M its place on December 12th; N its place December 21st; O its place December 29th; P its place January 5th following; Q its place January 25th; R its place February 5th; S its place February 25th; T its place March 5th; and V its place March 9th.

From this figure it is readily seen that the perihelion distance of the comet at V on March 9th, after which date it became invisible, is about double its perihelion distance at I, when it first became visible; and, from this we readily infer that although twice as far from the sun at V than at I, still the intensity of its light at the former place was, at least, equal to the intensity of its light in the latter place. On the reflection theory the intensity of the comet's light at V ought to have been one-fourth part only of that at I; and, therefore, the comet ought not to have been visible at all at V.

This fact, then, positively disproves the reflection theory. And, if not reflection, what then? Heat is the only alternative.

11. So far, I think, have we succeeded in showing that our theory at once and in a most satisfactory manner does away with the first four problems enumerated in Art. 4 of this section. We will now turn our attention to the fifth problem, and we suggest the following solution:—

Any molar motion of one small part of a moving body, with respect to the other and main part, caused by molecular forces (heat), must, other things being equal, take place in the direction of the original motion; for only in this direction can the whole of the molecular energy (heat) causing the motion be completely converted into, or spent for, the molar motion produced—molar motion in any other direction involves a deflection of the part moved from the straight line of the original motion and must, therefore, according to our theory, be accompanied by the development of heat in the part moved, so that, a part of the molecular energy (heat) causing the molar motion, will reappear as molecular energy (heat) in the part moved, and, consequently, as far as that part of energy is concerned, there will ensue no change from molecular to molar energy; but the tendency of the molecular energy is, by supposition, for some cause or other, to produce molar motion; therefore, will the motion produced be in the line of the original motion.

Now, in the comet, the cohesive resistance offered by the liquid or gaseous shell or envelope to the expansive force of the excessively heated central mass is the same in all directions; so, also, is the resistance offered by the weight of this shell the same in all directions. And, therefore, does the molar motion resulting from this expansive force take place by preference in the direction of the original motion of the comet. Thus the mystery is explained through and *by* our theory, which, in turn, derives additional proof from this fact also.

12. Problem VI of Art. 4 is the one to which the greatest mystery connected with the phenomena of comets is attached, and for the explanation of which the special aid of a new and unfamiliar force, a repellent force, residing in the sun, or inherent in its rays has been invoked. Therefore it is with great timidity and almost with reluctance that I am attempting a solution of this problem. And I say, the erupted matter moves away from the sun simply because it is ejected in the direction of the original motion of the comet;

and were it ejected in the contrary direction, it would move toward the sun and the tail would be directed to the sun and not away from it (and, as a matter of fact, some tails are really directed toward the sun). The reason for it is the following:—

If the velocity of a body (say the moon) revolving around a certre (say the earth) be increased through some other cause than central gravitation—the first effect of the increase of velocity will be a recession of the revolving body from its centre, or, in the language of the astronomers, the mean diameter of the orbit of the revolving body will be increased; and this is equivalent to saying that the revolving body will in the first instance move away from its centre; the second effect will be a diminution or the original velocity of revolution of the revolving body, for with an increase of distance from the centre there must necessarily go a diminution of velocity of revolution. This is a very well known and firmly established principle in physical astronomy, and we need not dilate on it any further.

Now, for its application to our case. The matter erupted from the central part of the comet is ejected with great violence, and, therefore, it possesses considerable velocity in the direction in which it is ejected relative to the main body of the comet; and since the gravitational attraction of the comet on the erupted mater is comparatively small, therefore will the erupted matter begin to revolve around the sun in an orbit of its own, separate and distinct from the orbit of its parent—the comet. It will continue thus to revolve around by itself for some time, until, through the gravitational attraction of the comet, it will finally sink into it. Now, since this erupted matter is ejected in the direction of the motion of the comet, it follows, that the velocity of revolution (around the sun) of this erupted matter is *increased,* and, therefore, by the principle above stated, it will recede from the sun and move in an orbit larger than, and circumscribing, the orbit of the comet; and this at once, so it seems to me, explains the whole mystery. The formation of the tail out of successive portions of this erupted matter and its gradual absorbtion into the body of the comet by gravitation, are subjects only to be mentioned here.

Two facts should be especially borne in mind, in order to obviate any objection to this solution of the 6th problem:

(a) The great violence and velocity with which the central matter is ejected from the comet.

(b) The comparatively small attractive force of comets, because of their small mass.

13. Although it is not our province nor our object to explain all phenomena connected with comets, still, as it will greatly conduce to our purpose to remove all mystery from the domain of comets and bring them within the pale of the ordinary laws of matter and force, therefore will we attempt to discuss and offer an explanation to the following mysterious phenomena.

(a) "A curious phenomenon, not yet explained," says Professor Young, "is the dark stripe which in a large comet nearing the sun, runs down the centre of the tail, looking very much as if it were a shadow of the cometic head. It is certainly not a shadow, however, because it usually makes more or less of an angle with the sun's direction. . . When the comet is a greater distance from the sun, this central stripe is usually bright." (Elements of Astronomy.)

(b) Another unexplained phenomenon is the following:—As is well known, the activity of the comet increases with its approach to the perihelion. Not only does its luminosity increase, but its tail also lengthens and becomes more defined. Most comets, indeed, shine with their greatest splendor and present the maximum length of tail immediately after perihelion passage—a fact most satisfactorily accounted for by our theory. So it is with most of the comets. But some great comets (that is to say, as we have explained, those that have the greatest periods of revolution and which in consequence move with great velocity near perihelion) form an exception to this rule. After perihelion passage they become "shorn of their splendor" to a great extent, their tails diminish, or are entirely lost. Halley's comet of 1835 became entirely invisible after perihelion passage for more than sixty days.

"As this comet approached the sun, its tail, far from increasing, diminished; and between the middle of November and the 21st of January, strange to say, both head and tail were altogether destroyed or, at least, rendered invisible. On the 21st of January the comet was actually seen as a star of the 1st magnitude without any tail or haziness, and was only known not to be a star by being exactly in its calculated space, and by its not being there next night. After that its head seemed to form again round this star and grew rapidly and visibly from night to night . . . This growth of the comet was so very rapid that in the interval of 17 days from the time I first saw it as a round body its real bulk had increased to 74 times the size it then had." (Sir John Hershel's *Familiar Lectures.*)

Enke's comet, a small comet, has also been noticed to diminished in volume at or near perihelion and to increase on its recession from it.

Sir John Hershel offers as the only possible explanation of these phenomena the assumption that a portion of the substance of these comets is evaporated by the sun's heat, and is thus converted into transparent non-reflecting gas, which, when the comet moves away from the source of heat—the sun—recondenses into cloudy vapor, which again reflects the sun's light and becomes visible. Now, Halley's comet at perihelion is 55 million miles distant from the sun, that is to say, its distance at perihelion is more than half the distance of the earth from the sun. The intensity of the sun's heat at that distance is therefore less than four times the same intensity at the earth's distance; and it, therefore, becomes extremely difficult to see how the (nucleus) head of the comet can be evaporated by so small a heat, even considering it as constituted of water. But as a matter of fact, as evidenced by certain meteors, which are (positively) nothing else but fragments of comets, the temperature necessary to evaporate the substance of comets and convert them into gas must be very high. The distance of Mercury from the sun is only about 36 million miles—still Mercury, which is thus *constantly* exposed to a greater heat than Halley's comet is for a brief period at perihelion, has not been, and is not at present being, nor will it ever in the future, be thus evaporated.

14. Now, with our theory, the explanation of Sir John Hershel of the phenomena witnessed especially in Halley's and Enke's comets becomes rational; for our theory stands in no need of the rays of the sun as a source of heat; and, therefore, it may be easily conceived that at perihelion the heat developed in Halley's comet was sufficient to *dissipate* the entire comet, and that in Euke's comet the same heat was sufficient to *dissipate* a part of the comet.

But here a difficulty arises, namely: how can any "dissipation" by heat render any incandescent body invisible? By "dissipation" you can mean nothing more than the conversion of a solid or liquid body into a highly attenuated gas, or the conversion of a gaseous body of any density into a gaseous body of smaller density. Now, if this "dissipation" is done by heat, the incandescent body thus "dissipated," so far from becoming invisible, ought, on the contrary, emit rays of light and heat of greater intensity than before it was so dissipated. Shooting stars, let it be remembered, must necessarily be looked upon as incandescent bodies, and their vanishing at some 50 or 60 miles has also been accounted for by the very same "dissipation." Our difficulty, therefore, embraces shooting stars also. By "dissipation" a shooting star ought to become more luminous and larger than before.

15. To rectify this apparently insurmountable difficulty, let me suggest the following analogy:—

Sound, as is well known, is produced by vibrations or undulations of air (or any other medium); and the greater the rapidity of these undulations the more intense is the sound, or, as we say it, the higher the pitch; and the smaller the rapidity of these vibrations—the lower the pitch. But there is a minimum and a maximum to this rapidity of vibration, both of which, the minimum and the maximum, produce no effect on the auditory nerve, and no sound is felt. Thus, a frequency of vibrations less than 16 per second has been ascertained to produce no sound at all; similarly, a frequency of vibration more than 33,000 per second cannot be felt by human ear as sound.

Now, light is also produced by ethereal vibrations, and the different colors of the spectrum correspond to different frequencies of these vibrations. A certain number of vibrations is felt by the optic nerve as red light; but a frequency of vibration less than this certain number is not felt as light at all. Another certain number of vibrations per second is felt by the optic nerve as violet light; but a frequency of vibrations more than this certain number does not effect the ratina at all, and is not felt as light. Orange, yellow, green, blue, and indigo light correspond to different frequencies of vibrations (in ascending numbers) intermediate between those for red and violet. As a matter of fact, the solar spectrum has been ascertained to extend beyond both the red and violet bands as invisible spectra: beyond the red as the *heat* spectrum, and beyond the violet as the *actinic* or *chemical* spectrum. Now, when a body is sufficiently heated, it at first becomes red hot—the vibrations of the molecules are the lowest in the scale of light. Further heating causes some of the molecules to vibrate with the frequency coressponding to orange light; still further heating causes some of the particles to vibrate with a "yellow" frequency and the incandescent body assumes a yellowish color; still further heating causes some of the molecules to vibrate with a "green" frequency, and the color of the incandescent body corresponds to a composite color of red, orange, yellow, and green. Further and further heating will cause some colecules to assume in succession the blue, the indigo, and violet frequency; so that finally the color of the incandescent body will correspond to a composite color of red, orange, yellow, green, blue, indigo, and violet—while light.

What will occur on heating the body further? Necessarily this. Some of the molecules will entirely cease to vibrate within the light scale and will begin to vibrate with a frequency higher than the violet—chemical or actinic frequency; and those molecules which will continue to vibrate within the light limit will also have their frequency of vibration increased, so that a greater number of molecules will vibrate with the "violet" frequency and a smaller number with the "red" frequency—and the body will emit bluish light. Further and further heating of the body will result in a greater and greater number of molecules leaving the *light* scale altogether, and vibrating with the actinic frequency; while the remainder of the molecules will nearly all vibrate with the highest frequency within the limit of the light scale—violet—and the incandescent body will become decidedly blue. Finally, on still further heating of the body, we may conceive *all* or *most* of the molecules to leave the light scale altogether and to vibrate with a frequency above the violet—the maximum frequency of the light scale—and the body will then become invisible: it will entirely cease to emit light rays, and its rays will consist *wholly* or *mostly* of actinic rays, or perhaps of rays of a still higher frequency, of which we know no name.

Some physicists would have it, that, in a body progressively heated, light rays of higher refrangibility are graually superadded upon those of lower refrangibility, but do not supplant them. But this appears to us impossible; for one and the same particle of matter cannot be conceived to vibrate with two or more different frequencies simultaneously and at once. We are, therefore, constrained to attribute rays of different refrangibilities to different molecules, and, if so, the rays of higher refrangibility must necessarily, by degrees, supplant those of lower refrangibilities, and, therefore, a degree of heating may be conceived when all light rays are supplanted by rays of higher refrangibility, and the body becomes invisible.

16. True, on earth we have no example of an incandescent body becoming invisible through progressive heating; but this is merely because the degree of heating necessary to render the body invisible cannot be attained here on earth, for obvious reasons. But the heat of the comet depending, by our theory, on the velocity of the comet and the intensity of solar gravitation conjointly, may be conceived to become so great at perihelion as to suffice to render a part or the whole of the comet's substance invisible, and this is how we explain the volume diminution of Euke's comet at perihelion and the entire disappearance of Halley's comet at and near perihelion in 1836.

Remark—We said, "On earth we have no example of an incandescent body becoming invisible"—no perfect example, indeed; but we do have an excellent partial example. It is the oxy-hydrogen flame. As is well known, this flame is the *hottest* flame known—that is to say, it possesses and emits more heat energy than any other flame (other things being equal). Now, the oxy-hydrogen flame is also the least luminous of all flames; it is also the most actinially potent of all flames—in perfect accordance with what was said in the previous article.

It is almost superfluous to add that the part of the comet first attaining the heat degree of invisibility must be the most central part within the nucleus; and as this is ejected, the visible part of the nucleus becomes diminished in size; and, as part after part is thus ejected in an invisible state, the whole of the comet is gradually consumed and becomes entirely invisible, and that when the comet moves away from perihelion and begins to cool the process is reversed: The most exterior part of the comet cools off first to within the light scale, but as in cooling it becomes heavier than the central part of the comet it sinks to the center of the comet, and gradually this central luminous center becomes increased in volume by the gradual sinking of luminous matter from the surface, and so a comet "grows" from a central spot.

17. The mysterious dark stripe which, in a large comet nearing the sun, runs down the centre of the tail and which, when the comet is a greater distance from the sun is usually bright—this "stripe" becomes at once comprehensible by the *invisible hot matter* theory. For the central part of the tail is, for obvious reasons, the hottest part of the tail, and, therefore, when the comet nears the sun, it may be conceived to become so overheated as to become dark and appear through the transparent *luminous* matter of the tail as a shadow; and when the comet recedes from the sun this overheated central stripe gradually cools off and becomes highly lumnous—"bright."

18. We thus see that our theory at once and most satisfactorily explains each and every phenomenon connected with comets, and all of them conjointly. It, our theory, is, I might say, the wonderful key which opens all the locks of mystery connected with this subject. It is the magic wand, at the touch of which all darkness, confusion, disorder, mystery, and ignorance vanish to make room for light, harmony system, understanding and knowledge for ever!

Is there in the realm of knowledge any truth of nature more positively and emphatically asserted by nature herself, through her own facts, than this, our grand truth? A truth which renders intelligible so many diversified and otherwise mysterious and contradictory facts of nature: of the earth, of he planes, of the meteors, and especially of the wonderful phenomena of comets; a truth which at once removes *all* mystery and apparent contradiction, and introduces harmony and unity in so vast a field of nature; a truth which brings the earth, the planets, the meteors, and the comets together, and correlates them one to another, subjecting them to one and the same single law—such a truth cannot but be a most positive and grand truth! We, therefore, hence forward consider our theory as perfectly and unshakably established; so that we will allow ourselves to employ this theory, in what comes, partly as an established principle on the strength of which certain inferences may be drawn.

SECTION V.—THE SUN.

1. It is with great trepidation and a feeling of sacred awe that I approach this grand subject of the SUN, the grand central orb and the mighty sustainer of the worlds, which, taken together, contribute the solar system. The vastness and grandeur of the sun, his importance for, and great dominating power over the worlds which it controls; his being the only source of energy, physical and vital, on earth; and above all, the great mystery surrounding all the phenomena exhibited by him—are subjects which only the extremely phlegmatic, the dull-minded, and unimaginative, can pass without experiencing this peculiar and rather pleasurable and undefinable feeling of "sacred awe."

Even before the days of universal gravitation; before the days of the telescope and spectroscope; before the days of chemistry and physiology—even then men, intuitively, and, I might say, instinctively, felt and were aware of this great importance of the sun to them. They left that were it not for the sun, life and the existence of living beings, beginning with the lowest vegetation and ending with man, could not exist on earth. They did not *know* it, but felt. By a most inexplicable law, where knowledge is wanting respecting things touching on life and existence, intuitive feeling, instinct—or call it by whatever name you please—takes the place of knowledge.

No wonder, therefore, that many nations worshiped the sun as a deity, and that all mankind see in the sun a symbol of the deity.

To us in particular is this subject of the sun especially awe-inspiring, for we are about to say certain things of the sun which are certain to startle our readers and which will at first sight appear highly incredible and almost ridiculous. And we dread the censure of mankind if those things be not true; and we are filled with reverential awe at the importance and grandeur of the revelations (for such they are, revelations) arrived at through and discovered by the aid of *our theory*—if those things be true, as we firmly believe, or rather are convinced, they are.

2. The phenomena displayed by comets are, indeed, mysteries according to the old conception of things; but when compared with the mysteries surrounding and enveloping the manifestations of the sun, they vanish into insignificance.

The first grand mystery presented by the sun—a mystery which outweighs all mysteries of physical nature taken together, and which Sir John Hershel rightly terms the "mystery of mysteries," is the positive fact that the sun continually and steadily radiates away into space enormous quantities of heat and light without any apparent diminution of heat in the body of the sun itself; in other words, the sun is being cooled off very rapidly indeed, and, nevertheless, in spite of this enormous cooling, it never grows cooler, and continues to give out the same quantity of heat and light from century to century. This wonder of the sun is something analogous to the wonder beheld by Moses when God first manifested Himself to him: "And behold, the bush is burning with fire, and the bush is not consumed." (Exodus.)

3. The sun, as is very well known, is an enormous globe of matter similar in all respects to the matter which we see around us here on earth, or to that constituting the planets; with this difference only that the

matter of the sun is in a very highly heated condition, more so than the matter constituting the earth's globe of that of the planets. And, if in the case of the earth a cause was necessary to account for the heat *found* in *its* interior, more so is a cause imperatively required to account for the heat *found* (or present) in the sun.

4. The physicists, absolutely unable to assign any natural cause for the *presence* of this heat, have tried their utmost ingenuity and skill to invent and to guess what *might have* been the cause, but their honest and indefatigable labors seem to have been far from having been crowned with success. For, some of them, in utter despair, were compelled to resort to the nebular hypothesis of Laplace as an *explanation* of this heat; but Laplace, as was pointed out, does not *explain* this heat; he *assumes* it, and that is all. Others, anxious to give some explanation, *assumed* things unnatural and fanciful, as the existence of a hot region of space, the collision of meteoric masses, catastrophes of one sun colliding with another, and so on and on. One thing becomes evident—the situation appeared hopeless to these great, true and indefatigable servants of science and of mankind; and, therefore, like a drowning man catching at a straw, they clung to anything that offered some appearance of a rescue from the situation. As to the value of these hypotheses, I refer the reader to art. 3 of our section on the earth.

5. But a still more formidable difficulty had to be encountered by men of science with reference to solar heat. It is this: With the earth's interior heat, it was sufficient to lull and pacify the restless mind somehow as to the cause of its *presence,* and then we could easily go on and say that the heat is actually going on dimin-

ishing by conduction and radiation and otherwise; no one could (easily at least) refute our statement.

Not so with the sun. The quantities of heat-and-light energy radiated away by the sun in each and every second of time are so enormous that, were the sun merely a hot incandescent body in the process of cooling off ,as the earth is supposed to be, the effect of this cooling could not but be easily perceptible or detectable in the body of the sun itself, or in a diminution of the amount of heat and light radiated by him; and *that* within a comparable short period of time—say one hundred years.

According to the latest and best estimates of solar radiation (Laugley's), each and every square foot of solar surface radiates away into space, in every second of time, an amount of heat energy equivalent to 6,600,000 foot pounds; but a square foot of solar surface corresponds to a mass of the sun's substance equal to 66,000,000,000 pounds, or thereabout; that is to say, then, that each and every pound of matter of the sun's substance loses in one second $\frac{6,600,000}{66,000,000,000}$ foot pounds or $\frac{1}{10,000}$ foot pound. Now, as there are in one year about 31,000,000 seconds, each and every pounds of the sun's substance loses in one year heat energy equal to $\frac{31,000,000}{10,000}$ foot pounds or 3,100 foot pounds.

But 3,100 foot pounds of energy will be more than sufficient to raise the temperature of a pound of water at 60° F by full 4° F; we, therefore, infer that, even considering the sun's specific heat to be that of water, the temperature of each and every pound of the sun's substance ought to be reduced in one year by fully 4°

F.; in one hundred years, therefore, each particular pound of substance of the sun must be reduced in temperature by 400° F. Now, the maximum estimate of the effective temperature of the sun's surface (Rosetti's) is 8,000° F.; therefore, ought the surface temperature of the sun be reduced in 100 years by more than two per cent; and the amount of heat and light radiated by that surface ought correspondingly be diminished by more than two per cent; and in 5,000 years the surface of the sun ought to grow "cold" and cease radiating almost altogether!

That such is in reality not the case need hardly be said. Chinese history, we are told, extends to more than five thousand years backwards, and the Chinese at that time lived and wrote history and studied astronomy without the slightest inconvenience from the scorching hot rays of the sun. Neither can one of us believe that in 5,000 years from now the sun will grow cold and dark, and all motion, life and thought cease upon earth!

"We may regard it as good as an observed fact," says Professor Newcomb, "that the sun has been radiating heat into void space for thousands, or even millions, of years without any apparent diminution of the supply. One of the most difficult questions of cosmical jhysics—a question the difficulty of which was not seen before the discovery of conservation of force—has been how this supply of heat is kept up. If we calculate at what rate the temperature of the sun would be lowered annually by the radiation from its surface we shall find it to be $2\frac{1}{3}$° F. per annum, supposing its specific heat to be the same as that of water, and from 5° to 10° F. per annum if we suppose it to be the same as most of the substances which compose our globe. It would therefore have entirely cooled off in a few thousand years after its formation if it had no other source of heat than that shown by its temperature."

That the amount of light and heat emitted by the sun, and, consequently, its surface temperature has been constant and practically stationary for at least the last three thousand years—of this we have sufficient evidence in the unaltered distribution of plants over the surface of the earth.

"In order that the *date palm* should ripen its fruit the mean temperature of the place must exceed 70° F.; and, on the other hand, the *vine* cannot be cultivated successfully when the mean temperature is 72° F. or upwards. Hence, the mean temperature of any place at which these two plants flourished and bore fruit must lie between these narrow limits—i. e., could not differ from 71° F. by more than a single degree. Now, from the bible we learn that both plants were *simultaneously* cultivated in the central valleys of Palestine in the time of Moses, and its then temperature is thus definitely determined. It is the same at the present time, so that the mean temperature of this portion of the globe has not been sensibly altered in the course of thirty-three centuries." (From an address delivered by Professor Humphrey Lloyed in 1857 before the British Association for the Advancement of Science.)

Now, this fact implies that three thousand years ago Palestine was receiving as much heat and light from the sun as it now does, and we may, therefore, safely infer that the surface temperature of the sun has not been lowered in any sensible degree through all these long years, and that the quantity of heat in the *sun* is as great now as it was three thousand years ago, in spite of the fact that during these years the sun lost more than one-half of the heat it possessed three thousand years ago. "The bush is burning with fire and the bush does not get consumed."

In a word, while the constancy of the earth's central heat is not superficially apparent, and has to be inferred, as was said in the section on the earth—the constancy of the sun's heat, in spite of enormous losses, is directly and immediately apparent to the most uncritical observer.

6. The physicists have grudged neither time nor labor, and have employed their best skill and inventive power, in order to unveil this great mystery of nature, but apparently with little or no success. Numerous theories, hypotheses and ingenious guesses have been from time to time made by various great men with this purpose in view, but all of them, after enjoying a longer or shorter period of vitality as current opinion among the scientists, had finally to be rejected as grossly erroneous or inadequate. Among these the so-called meteoric theory, first originated, I believe, by Myer, deserves special mention. Acording to this theory, the mysterious heat supply of the sun is maintained by the destruction of vis viva of numberless meteors constantly pouring with great velocity upon the surface of the sun! This theory, fancifully wild and chimerical as it is, has, strangely enough, enjoyed for a long while the widest acceptance among scientists. The physicists actually entertained the notion that, for millions upon millions of years past and to come, a ceaseless rain, shower, or hail of stones, large and small, has been, is and will be, going on upon the surface of the sun. And it is upon this rain that change and motion, life and growth, thought and feeling, upon the surface of the earth (and planets) depends!

Whence this rain? Where are the clouds giving rise to it? And may not this rain cease to-morrow and an end take place to all our mental activity?

These and such like were considered as puerile questions and unworthy of consideration. The physicists, as I said, were in a state of mental distress and despair, and, when something offering apparent relief from this state was offered to them, they eagerly and without stopping to consider accepted it. But at last, cool and calculating reason gained supremacy, and this theory, like a great number of its predecessors, had to be discarded and relegated to the past. Lord Kelvin, at one time a staunch supporter of the meteoric theory, finally had to acknowledge its inadequacy and prove its impossibility from the undisturbed motion of the planets. (See Popular Lectures and Addresses, by Lord Kelvin. Vol. 2, pp. 127-121.)

7. The meteoric theory dethroned, another, and, I say, equally untenable theory took its place. I refer to the well-known contraction theory of Helmholtz, which is accepted with much confidence by some and with much more hesitation and reserve by others, According to this theory, to put it briefly, the loss of heat by the sun cools it; but this cooling entails a contraction of the sun's mass, or, in other words, an approximation of the particles of the sun's substance nearer the centre; and, as this entails a loss of potential energy, the maintainers of the contraction theory think that the potential energy thus lost by the hypothetical contraction is converted into heat, and it is this heat that supplies *that* radiated away by the sun.

This contraction theory of Helmholtz is in fact but part and parcel of the old Kantian nebular hypothesis, which accounts for the necessary heat of the nebula by its contraction, and which we so fully discussed in our INTRODUCTION to our "Evidence." I urgently request the reader to reread our sixth criticism of the nebular hypothesis, from which it will at once become apparent why I so boldly and unhesitatingly called this contraction theory of Helmholtz *untenable.*

I don't know whether Helmholtz, while formulating his contraction theory, was or was not consciously aware of the fact that the sun was, is, and will be, rotating from west to east with a certain speed (25 days, or so). But *we* do know it as a positive matter of fact, and, therefore, we may boldly and assuredly say that the theory is "untenable." For any potential energy lost by the contraction of a rotating body, no matter what the cause of this contraction, must positively appear as kinetic energy in an increase of the speed of rotation, as demanded by the positively true law of "conservation of areas," and can, therefore, not appear as heat. It is, indeed, greatly surprising how the great physicists and astronomers, who are so thoroughly conversant with the laws of motion, could ever have dreamed of accepting the contraction theory, as affording an explanation of the sun's heat supply. But, in justification, I might perhaps say that the urgent necessity for an explanation of this strange fact, and the great difficulty of getting at one, might have blinded their vision to any objection against this theory. Be it as it may, one thing is certain—either the law of "conservation of areas" is false, or the contraction theory of Helmholtz is false and untenable. The verity of which do you prefer, dear reader? Assuredly the verity of conservation of areas, I may safely answer for you.

Further, the contraction theory, even if tenable, is inadequate; for, no matter how the physicists may stretch and strain their figures, and no matter how far expanded the sun they may suppose to have originally been, it nevertheless comes out according to this theory that the sun could not have illuminated the earth for more than 18,000,000 years, a period confessedly insignificant for the existence of the solar system, as is to be inferred from positive geological records and otherwise. (See section on the Eearth, art. 7.)

We will, therefore, conclude by saying that the sun's heat supply is even at present, just as it had been in the past—a great mystery, indeed "the mystery of mysteries."

8. We will leave the subject of the sun's light and heat without further comment for the present, and betake ourselves to other remarkable and, at present, mysterious phenomena displayed by the sun. And the first to call our attention are the solar spots. Ever since the days of Galileo, these solar spots engaged the attention of many great astronomers and physicists, some of whom devoted large portions of their lifetime to a careful study and observation of them, in order to divulge the secret hidden behind them. But no success followed. On the contrary, the mystery deepened and assumed larger proportions the further the study of these spots was pursued, and the more the physicists became familiar with their characteristics and features. And at the present we are (I say it unhesitatingly and convincedly) much further from a solution of the nature and causes of these spots and the various phenomena connected with them than was Galileo, their first discoverer; and this in spite, and perhaps because, of the persistent and laborious researches of Shwabe, Faye, Sechi and many other great men.

9. As our intention is other than to write a treatise on astronomy or a minute description of the sun, we will, therefore, earnestly request the reader, who may not be familiar with the subject of sun spots, to take up any good modern book on astronomy and familiarize himself before proceeding further with the following subjects:

(a) The structure of the spots, umbra, pemumbra and faculae surrounding the spots.

(b) The mode of their formation and disappearance.

(c) Positive eruptive phenomena accompanying the spots.

(d) The distribution of the spots.

(e) Proper motion of the spots.

(f) Their periodicity.

(g) The equatorial accelleration of the sun.

(I can heartily recommend for this purpose the admirable work of Professor Young—"The Sun.")

10. Now, as to the nature of the spots and theilr causes, we are at present nearly as far from a solution as was Galileo and others, three hundred years ago. The number of theories that have in the past been proposed, and that are now prevalent, as current opinion, purporting to explain the nature, appearance, and causes of the spots, is indeed legion, but they are all merely hastily thrown-out conjectures without any solid foundation to rest upon. More, they are not only not free from objection, but are entirely and wholly objectionable and out of all harmony with the regular nature of things. Among these I include also the ingenious hypotheses of Faye and Sechi, the objectionable points of which have been so ably pointed out

by Professor Young. Young's own hypothesis is not much better.

Now, spots (similar to the solar spots in all respects excepting optical appearance) are not peculiar to the sun alone. They have been repeatedly observed on some of the planets, especially on Jupiter and Saturn. and they behaved there exactly as those of the sun—made their sudden appearance, lingered for a longer or shorter interval of time, and then disappeared. So that we have the strongest possible reason to suspect that what constitutes the nature and cause of the planetary spots must also constitute the nature and cause of the solar spots.

Now, to my mind, it seems that there can hardly be any doubt that the spots observed on Jupiter and Saturn represent openings on the surface of those planets, caused by volcanic eruptions similar to those occurring on earth. Indeed, all through the sections on the planets I assumed them to be such. The idea that the spots of Jupiter and Saturn are due to clouds, must at once be rejected, when the fixity of position of these spots is taken into consideration. No amount of argument will succeed in persuading any one that clouds should be fixed in one position in such a tumultous atmosphere as that of Jupiter for months and years. More, a cloud in the atmosphere of Jupiter is not a thing to be remarked at all, for Jupiter's atmosphere is constantly full of them. More, the fact that the spots of Jupiter occur mostly at the perihelion passage of the planet, as was pointed out above, makes it almost certain, in accordance with our theory, that they are caused by increased thermal activity (volcanic eruption). The observation of Hall on December 7, 1876, mentioned in art. 10 in the section on planets, makes it certain that on that date a volcanic eruption took place near the equator of Saturn and was the cause of the brilliantly white spot.

Now, if such be the nature and cause of the planetary spots, the nature and cause of the solar spots must, by the law of uniformity of nature, be the same; that is to say, they must be openings in the photosphere caused by eruption brought about by the imprisoned central heat of the sun. Wilson of Glaskow has positively and absolutely demonstrated that the spots are depressions, cavities in the solar substance. And is it not more logical and natural to suppose that these cavities have been formed by the unbalanced superior force of the centrally imprisoned heat, which must tend to produce motion outward, than that they are formed by eddies, cyclones or whirlpools, produced by an inadequate force due to a hypothetical and unexplained uneven drift of photosphere (the equatorial accelleration), which is Fay's theory? or that they are produced by cooled off metallic vapors which, becoming heavy, drop into the photosphere and produce depressions, which is Sechi's theory? More, the mode of the formation of the spots clearly shows, so it seems to me, that they are formed by forces from underneath the photosphere.

"Generally," says Professor Young, "for some time before the appearance of the spot there is an evident disturbance of the solar surface, manifested especially by the presence of numerous and brilliant faculae, among which "pores" or minute black dots are scattered. These enlarge, and between them appear grayish patches apparently caused by a dark mass lying veiled below a thin layer of luminous matter. The veil grows gradually thinner and vanishes, giving us at last the completed spot with its perfect penumbra." (*The Sun,* p. 22.)

Is this mode of formation, we ask, in harmony with either Faye's or Sechi's theory, and is it not directly contradictory to these theories, or, for that matter, to any theory now prevalent? Is it not clear as daylight that the dark solar spots are "gradually unveiled" by forces from underneath? More, the presence of faculae surrounding the mature spot, and also their presence during their formation, clearly shows that an eruptive, elevating process is going on *in* and *around* the spots, for faculae are positively elevations and not depressions.

More, the spectroscope has positively demonstrated to us the fact that frequent outbursts, or eruptions, of darkly luminous matter takes place from the spot or its vicinity.

"It must be admitted," says Professor Young, "that in some cases the appearance puzzlingly resemble the emergence of a dark mass *from beneath.*"

What more do you want? Is not our case clear? And is it not rather to be wondered, how it happens that the physicists have left this terra firma of solid facts to wander out into the realm of imaginary processes and causes?

11. What is the difficulty? I mean, for what reason is it that the physicists failed or were unwilling to recognize the fact that the spots are openings caused by solar volcanic activity? There are two reasons for it: 1. The color of the spot. If the spots be caused, as we say, by the excessively heated gases of the interior, which force their way out, as does lava here on earth—then, ought the spot itself, being a cavity filled with matter hotter than the photosphere, be also more luminous than the photosphere; and the spots ought, therefore, to appear bright and not dark. A diminu-

tion of luminosity is associated in our minds with a diminution of temperature, and therefore it is that the physicists have all striven to account for the dark spots as due to something colder than the photosphere. So did Sir William Hershel, who on this very account assumed the central mass of the sun to be a cold, solid body peopled with inhabitants. So did also Sechi, as we have just seen. So also did Faye, with his "sucked-in," cooled off and absorbing gases.

The second and perhaps more difficult objection is constituted by the spectroscopic appearance of the umbra of the spots. Under high dispersive spectroscopic power, the spectrum of the umbra is found to consist of widened dark lines, which ,ordinarily, is really an absorption spectrum and not a spectrum of heated gases, for a spectrum of heated gases should consist of bright lines. (See Note A at end of this section.)

12. When one feels certain that the truth rests with him, he naturally and unconsciously becomes bold. Being, therefore, convinced that the truth rests with us, we will dare to present our view of the matter.

The more superficial layers of the sun, being, for obvious reasons, cooler than the deeper layers, emit rays of lower refrangibility than the deeper layers (and therefore it is that the prominences, which are the most superficial layers of the chromosphere, appear red); and the further any one spherical layer of the sun is from the surface, the higher is the refrangibility of the rays emitted by it. At the photosphere, the whole scale of the spectrum is represented in full proportion, and it shines with an intensely white color. But the matter lying deeper than the photosphere being hotter, so to speak, than the photosphere, emits more rays of higher refrangibility than the violet and less rays of lower refrangibility than the violet; that is to say, it emits less light-rays and more actinic rays, and, therefore, it is less luminous than the photosphere; and the deeper we go below the photosphere, the greater the absolute quantity of heat energy in any given mass, and, therefore, the higher the refrangibility of its rays and the less luminous it becomes, until at a certain depth the matter is entirely devoid of light rays and the rays it emits are wholly of the actinic kind; that is to say, it is entirely non-luminous.

Another simpler exposition of the same idea is as follows: The luminosity of any heated substance increases with the increase of the absolute quantity of heat energy it contains; but this it does only up to a certain limit, and the further increase of heat energy beyond that limit has a contrary effect, and diminishes its luminosity, as ar as the capacity of our optic nerve is concerned. In other words, its luminosity becomes so intense that the retina is incapable of feeling this intense luminosity, just as in the case with sound. (See arts. 15 and 16 of Comets.)

Further, this overheated non-luminous matter is very transparent to foreign light, as pointed out in the above two articles.

13. And now we are ready with a full and most satisfactory solution of the solar spots. They are nothing else but the openings produced by volcanic eruptions. But the matter ejected in those eruptions, being overheated, is non-luminous and transparent; therefore, is the column of ejected matter rising above the photosphere entirely invisible, and what we see of the spot is only the light-deficiency of the interior of the sun. And, as the matter immediately below the photosphere

is more luminous than the matter further down, the appearance of the penumbra and umbra is thus explainable. The penumbra represents the matter rushing in upon the spot horizontally from the sides of the spot, right beneath the photosphere, which has some degree of light; while the umbra represents the matter rising vertically upward from great depth and piercing, so to speak, the veil that the matter rushing from the sides would otherwise form.

The following quotation from the Encyclopaedia Britannica will, Ithink, conclusively show that our explanation of the penumbra is correct, as the reader will understand for himself:

"When two spots lie very near to one another, the umbra (penumbra) is deficient on that side, which lies next to the other spot; and this will be the case, though a large spot should be contiguous to one much smaller; the umbra of the large spot will be totally wanting on that side next the small one. If there are little spots on each side of the large one, the umbra does not totally vanishes but appears flattened or pressed in towards the nucleus (umbra) on each side. When the little spots disappear, the umbra of the large one extends itself as usual."

It is obvious, that on that side of the spot where the other spot is, there will be less sub-photospheric matter to rush in upon either spot.

As I am no astronomer, and never used a telescope, I would rather leave the work of a detailed explanation of the umbra and penumbra, based upon this hot non-luminous matter theory, to the competent and skillful astronomer, fully confident, however, that with this theory in his hands he will have no difficulty in explaining all the appearances up to their minutae.

Thus is the first objection of the dark color of the spot entirely disposed of. It is dark not because the rays emitted by the spot are of a lower refrangibility than the red, but because they are of a higher refrangibility than the violet.

As to the second objection, that of the spectrum of the umbra, we might say that the dark lines are simply faded bright lines due to a deficiency of the light rays, or we might say that they are so intensely brilliant as to appear dark to us, for the same reason that the spot itself appears dark, according to our second exposition of the idea of hot non-luminous matter. But the best thing to say about the umbral spectrum is the truth—that as we are not familiar here on earth with such intense heat as to produce non-luminosity, therefore are we also not familiar with the spectrum of the various chemical substances at such high temperatures; and we, therefore, have absolutely no right to make any inference from such a spectrum. For, it is well known that the lines of any one substance change materially with the temperature of that substance.

As to our theory of hot transparent non-luminous matter, we wish to say that not only does it help us to a perfect and clear understanding of solar spots in perfect harmony with all the eruptive phenomena connected with them, but it has also other facts to rest upon—the vanishing of "dissipated" meteors is one; the disappearance and contraction of comets at the highest stage of their development is another, as was explained in articles 15-16 on Comets.

Besides, there are some inexplainable stellar phenomena that become at once intelligible on the application of this theory, as we will perhaps have occasion to point out when we come to deal of the stars. Our theory will also derive strong support from the mysterious connection of terrestial magnetism with the solar spots, as is well known. For, that the needle should respond to strong actinic rays sent out by the central matter of the sun is less to be wondered at than that it should be affected by "cooled-off" masses; besides, hot non-luminous matter may, for all we know, emit rays of still higher refrangibility than those we call actinic; they may emit "magnetic" or "electric" rays (for want of a better term). (See Note B at end of this section.)

14. So much for the nature and causes of the spots. But whether our explanation of the solar spots be correct or not, it is on all hands agreed that the development of solar spots is invariably attended with an increased thermal activity of the sun. What is the cause of this increased thermal activity? Why should the sun all of a sudden become more thermally active? This problem constitutes the second great mystery connected with the sun, which for the present we will leave unanswered. We will point out, however, the similarity of these questions to those of art. 11 on "Earth" about the cause of volcanoes on earth, which article we earnestly request the reader to carefully reread.

15. The third great, and to us most important, mystery of the sun, to which we will now turn our attention, is the wonderful periodicity of the sun spots discovered by Shwabe in 1851. It was the outcome of a 25-years' daily observation upon the sun for other purposes than that of discovering this wonderful and most important fact of periodicity. He is reported to have said of himself that, "like Saul, he went to seek his father's asses and found a kingdom." A kingdom, indeed, it is—nay, more than a kingdom, it is a world. And not only one World, but many countless Worlds. And these worlds not secondary and dependent worlds,

but primary and controlling worlds; not receiving worlds but giving worlds; worlds that sustain and give life and existence to all other worlds! For, this fact of the periodicity of the sun spots forces upon us and leads us to the revelation of a great hidden mystery of nature, a mystery the revelation of which at once solves many countless mysteries of nature, and among them the great mystery of the origin and source of energy and force, life and motion, change and growth!

16. These enigmatic expressions of mine will be rendered clear and intelligible in a subsequent page of this section. At present we wish only to state, that if our hteory be correct and the spots be openings in the photosphere caused by eruptions brought about by the sub-photospheric heat, it follows that the immediate cause of the greater number of the spots at the maximum must be a greater amount of imprisoned sub-photospheric heat, and therefore ought the temperature of the spots, os as to speak, be higher at spot-maximum than at spot-minimum. And if we do find such to be the case, it will constitute strong corroborative evidence of our theory of sun spots and also strong evidence against the theories of "cooled-off" masses. But we do find such to be the case.

It is very well known that the spectroscopic lines peculiar to any chemical substance, whether bright or absorption lines, change materially in number and position with the increase of temperature of the substance. Therefore, if the temperature of the spot be considerably higher than that of the photosphere and chromosphere, ought the lines of the various substances be much different in number and position in the umbral spectrum from those in the common solar spectrum; and therefore, also, ought the lines in the umbral spectrum at spot-maximum be different, in the same way, from those in the umbral spectrum at spot-minimum. And such is in reality the case.

The following quotation from the Encyclopaedia Britannica under "Sun" will show this at a glance:

"The most extensive sun-spot observations of this nature have been carried on in Kensington, and the conclusions derived from 700 observations on spots between 1879 and 1885 are as follows: (1) The spot spectra are very unlike the ordinary spectrum of the sun; some Fraunhofer lines are omitted; new lines appear, and the intensities of the old lines are changed.

. . . .

(11) Towards the end of the first series of investigations there appeared among the most widened lines a few which are not represented so far as is known among the lines seen in the spectra of terrestial elements. *This change took place when there was a marked increase in the solar activity*... .. ..

(12) The most widened lines in the sun spots change with the sun-spots period.

(13) At and slightly after the minimum the lines are chiefly known lines of the various metals.

(14) At an dslightly after the maximum the lines are chiefly of unknown origin.

(15) On the hypothesis under discussion the change indicates *an increased temperature in the spots at the sun-spots maximum.*"

17. But what is the cause of this periodicity? What is the cause of this undoubtful increase of sub-photos-

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pheric heat at the spot maximum and its diminution at spot-minimum? This is and has remained a mystery since the days of Shwabe. No one, as far as I know, has ever dared to suggest a solution to this mighty question, except it be the ridiculous idea that it is due to some mysterious influence of some or of all the planets revolving around the sun. Ridiculous it is indeed, because it means nothing and explains nothing, and attempts to explain the unknown by a still greater unknown.

18. The equatorial acceleration, and the limitation of the spots to two znes parallel to the equator, and their absence in the polar and equatorial regions of the sun, constitute two additional mysteries connected with the sun, which we will at present leave entirely untouched.

So, then, the mysteries of the sun, which stand out most prominently and which most imperatively cry out for a solution from scientific men, are:

(1) The average constancy of the sun's temperature and its radiation.

(2) The cause of the spasmodic and fitfful thermal activity taking place from time to time in the sun and manifested by sun-spots, which are caused by nothing else than by volcanic eruptions similar to those taking place on earth and on the planets.

(3) The cause of the regular and periodic increase and diminution of these spasmodic thermal activities—the periodicity of the sun spots.

(4) The equatorial acceleration.

(5) The distribution of the spots.

19. We have thus presented and discussed somewhat the great mysteries presented by the sun, excepting the mystery of the corona, with which we are not concerned.

Now, if *our theory,* aided by a little inference from analogy and otherwise, should at once and simultaneously offer a solution to all these mysteries, will not our theory thereby receive the strongest possible confirmation? And will not this bring *our theory* within the category of positively established truths?

But our theory, at this stage of its development, needs little additional proof for its verification, for we have sufficiently verified it by the phenomena displayed: (1) By the earth and the moon; (2) by the planets; (3) by the meteors, and especially (4) by the *comets.*

Therefore, the proper question to be asked is this: If a certain inferable fact, coupled with the application to it of our theory, should simultaneously and at once render *all* these mysteries of the sun intelligible, especially if that fact be not solitary but found in many instances of nature under similar conditions, will not this inferable fact be entitled to a most thorough consideration, even if it should appear most startling and novel in its application to the sun? Nay, more, is not such an inferred fact—a fact, as we said, positively existing in situations other than the sun—entitled to be accepted as a perfect truth and considered strong enough to combat certain doubtful appearances and certain doubtful inferences conflicting with this positively inferable fact? For my part, it is; and, before we are done with this section, we hope to convince the patient reader also that it is.

20. Many of the principles enunciated in this volume are indeed strange and novel, but, nevertheless, true. Doubly so will this fact, inferable on the strength of these very principles, appear "strange" and novel. Novel, indeed, for, as far as I know, no one has ever dreamt of such a fact. Still, in spite of all this, to me, the reality and existence of this fact is as clear and positive as is the reality and existence of the great, infinite, eternal, and inexhaustible force—*gravitation.*

To me it is not a doctrine, nor an hypothesis, nor a theory that I am about to enunciate, but a certainty, a reality and, in a word, a *fact.*

21. There are two classes of inferences: (a) An effect inferred from its cause; (b) a cause inferred from its effect or effects. Now, the truth value of the inference is apparently much greater in the former class than it is in the latter, for various and different causes may, each and every one of them, be antecedents of one and the same effect. But most of human reasoning, and especially scientific reasoning, consists mainly of inferences belonging to the second class. For with a given cause an effect must follow; and, therefore, if the cause be given, the effect is, practically speaking, also given; and there is, therefore, no necessity nor any use in an inference. The strength, beauty and grandeur of human reason lies, and can only lie, in its ability to ascend from effects to their causes; that is to say, in making true inferences of the second class.

Now, the truth value of an inference of this second class consists mainly in two things: (1) In the number of effects upon which the inference is based, for, evidently, the greater the number of effects, from which a crtain cause is inferred, the greater must the strength of the inference be; (2) the order and relation of the effects between themselves. If the effects be in such an order and bear such relations to each other as the inferred cause, if given, would necessarily produce, then, the inference possesses the highest degree of certainty.

Now, we claim that *our* inference possesses both of these qualifications. For first, it is based upon five effects, and secondly, the effects are such that if the inferred cause be given, they must necessarily follow.

But what is this inference?

22. In treating of the earth, we conceived the motion of the earth in a dynamical curve around the sun as a cause, and we calculated what effects must, in accordance with our theory, follow from this cause, and we found the necessary effects to be:

(a) The presence of heat in the earth.

(b) The average constancy of this heat through all time.

(c) Manifestations of a sudden character due to an accumulation of heat within the earth, volcanic eruptions and microsisms. (See art. 22. Earth.)

(d) A greater frequency and intensity of these manifestations at and near the earth's perihelion than in any other part of its orbit; that is to say, a "periodicity" of these manifestations.

We then proved that these effects do actually take place in the earth; that (a) the earth is hot; that (b) its average heat is constant; that (c) volcanic eruptions and "miscrosisms" do take place on earth, and that (d) these last have a periodicity of one year. And we, therefore, inferred that our assumed cause must be a true cause.

Now, when we come to examine the sun, we find the same four effects present:

1. *The sun is hot.*

2. *The sun's heat is constant in the average*

3. *Spots.*

4. *Periodicity of the spots.* L

Are we not, I ask the candid reader, to infer from these four cardinal effects, in the case of the sun, the same cause that we saw produces the very same effects in the case of the earth—namely, that these four effects are due to a motion of the sun in a dynamical curve whose form is an ellipse with a period of 11 years?

We hold, therefore, that *the sun, accompanied by all his retinue, revolves, within the vast interstellas space alotted to him, in a vast elliptical orbit, with a period corresponding to the spot periodicity* (11.1 years)—*around a great massive, cold, dark and invisible body, as a center.* And this is why the sun is hot and why its heat is constant in the average from generation to generation, and why, from time to time, spots appear on its surface accompanied by a multitude of eruptive phenomena, and why these spots have a periodicity of maximum and minimum—just as is the case with the earth and planets.

23. We have thus dethroned the sun from his seat of eminence as a primary centre of our system and have transferred this honor to the dark and invisible body around which the sun itself revolves in an elliptical orbit in a period of 11.1 years. We will, therefore, name our dark body "Primosol," the absolute and actual primary centre of the solar system; and the part of the sun's elliptical orbit, where the sun is at its least

distance from the*Primosol* we will term "Peri-primosol;" and the part where the sun is at its greatest distance from the Primosol we will term "a-primosol."

24. We have dethroned the sun, also, in another sense. Up to now the sun was looked upon as the original source of energy within the solar system, and to him the direct cause of life and growth, feeling and thought upon earth, were referred. This honor we have also mercilessly transferred from him to that mysterious, dark, cold and invisible *Primosol*. The sun, indeed, retains the office of dispenser of energy, but he is no longer the giver of energy. This last and supreme office appertains to the dark *Primosol,* for it is at the expense of the gravitational energy of the Primosol, which continually deflects the sun from the straight line of its motion—it is at the expense of this gravitational energy that the sun's light and heat is developed; and, therefore, is the Primosol the real giver of energy, life and thought, etc. "From the strong comes the sweet," said Samson, and "from the dark comes the light," let me say.

25. The cold and inert mass of the Primosol must greatly, very greatly, indeed, exceed that of the sun; so that its motion around the common centre of gravity is very small, and therefore it remains cold and inert, dark and invisible. Were the mass of the Primosol equal to that of the sun, the Primosol and the sun would both revolve around their common centre of gravity with the same velocity; and, therefore, as much heat and light as the Primosol would develop in the sun by its gravitational energy, just so much would the sun in its turn develop in the Primosol, and they would, therefore, both possess the same degree of luminosity. This

is the great secret of the binary systems of stars in the heavens, as we will have occasion to point out in a subsequent page.

26. Carried away, as it were, by the subject, we have slightly digressed from it and entered upon subjects foreign to the matter at hand. Now, to return. This inferred fact, it is hardly necessary to repeat, at once does away with the first four great mysteries of the sun. The sun is hot, because it moves in a dynamic curve. Its heat average is constant from eternity to eternity, because its average motion in that curve is constant from eternity to eternity. Sun spots occur from time to time on the sun, for the same reason that volcanic eruptions occur from time to time on earth. The sun spots are more numerous and of greater intensity when the sun is at or near its peri-primosol (maximum) than when it is at or near its a-primosol, for the same reason that microsisms have their maximum when the earth is at or near its perihelion and their minimum when it is at ornear its aphelion, and for the same reason that volcanic eruptions occur more frequently in the winter months than in the summer months.

In a word, the sun is, in these respects, a perfect prototype of the earth; and therein, let me say, lies the great grandeur and beauty of our theory that it correlates all the members of the solar system one to another, and points out that what occurs in the sun must occur in the earth and in the planets and in the comets, for they are all subject to one great eternal law.

For further and more detailed elucidation on these heads, the reader is referred to the "Earth."

27. So much for the first four great mysteries of the

sun. Mysteries 5 and 6 of art. 16, however, need a few words to be said before their solution, by the aid of our theory and the grand inferred fact, will be rendered apparent.

First, as to the distribution of the spots. Why should the spots occur mainly in two zones parallel to the equator between latitudes 10° and 30°? And why should they be entirely absent from the polar and the equatorial regions? We claim that our theory removes this difficulty at once.

The sun is subject to two motions—the motion of revolution around the Primosol and the motion of rotation around his polar axis. Now, were the sun non-rotating but subject to the translatory motion around the Primosol, spots would have to occur on or near the equator exclusively, and in the anterior part of the sun, as is the case with the eruptions of comets, and for the very same reason, as was explained in article 11 on "Comets," to which we refer the reader.

On the other hand, were the sun non-revolving but rotating around his polar axis, the eruptions of the central matter of the sun would take a polar direction, and spots would occur in the polar regions exclusively, for the simple reason that the matter near the poles possesses a smaller velocity of rotation than the matter near the equator, and therefore, offers less resistance to motion and change of direction than the equatorial matter. For matter with greater velocity has greater inertia, both motional and directional, than matter with less velocity, and, therefore, would the polar region be the place of least resistance to the expansive central forces.

But, since in actuality the sun is subject to these two motions simultaneously, therefore will the matter to be ejected by the central forces take a direction midway between the equator and the poles, in accordance with the law of parallelogram forces. For, the translatory motion of the sun directs it toward the equator, and the rotary motion of the sun directs it towards the poles, and the resultant of these two directions must lie somewhere between these two directions. It must also lie nearer to the direction of the stronger force, from which circumstance we may conclude that the translatory velocity of the sun is quite great, since the middle of the spot zone (lat. 20°) is much nearer the equator than to the poles. We may even hope that, in some future day, the mathematician will be able to deduce the sun's velocity from this position of the sun's spot zone.

28. The sixth and last great problem of the sun to engage our attention is the so-called equatorial acceleration. The spots were found to move with a uniform velocity across the disk of the sun from the solar west to the solar east, and from this it became obvious that the sun rotates as a whole from west to east in a period that could easily be determined from a complete revolution of any spot. But when this was attempted, it was found that spots perform complete revolutions in times varying with their distance from the solar equator.

"If we deduce the period by means of spots near the sun's equator we shall find it to be nearly 25 days, a trifle less, according to Carrington. Spots at a solar latitude of 20° have, on the other hand, a period of nearly 18 hours longer; at 30° the period rises to 26½ days, and at 45° to 27½ days." (*The Sun.* Young.)

A similar phenomena has been observed on Jupiter, whose period of rotation, it is to be particularly remarked, has also been deduced from spots like those o the sun. Be it remembered, also, that the cohesive state of Jupiter is nearly similar to that of the sun.

Now, how in the world could even a fluid body rotate with various periods for its various parts? Could we even artificially succeed in performing such a trick and start a fluid globe rotate with different periods for its different parts, the various motions of the various parts with reference one to another would speedily equalize themselves, and in a very brief period indeed it would be found that all the parts rotate with one period. This phenomena, therefore, constitutes a great, great mystery of the sun.

It would seem to me that, a priorily and without further discussion, we could summarily dismiss this subject, and state with positiveness, that the variation of the rotation with latitude is, in reality, anly apparent, and is due to some systematic displacement of the spots themselves with reference to the photosphere, which displacement varies with the latitude. It should be remembered that the spots, owing to the small cohesive resistance of the sun's substance, *have been* repeatedly observed to exhibit proper motions of their own.

"The rotation of the sun," says Rodwell, "as determined by the motion of the solar spots, would appear to vary according to the solar latitude, though, of course, in reality, there can be but one rotation period, and the difference actually observed are due to the proper motion of the solar spots." (Distionary of Science.)

What will be our astonishment, therefore, when we find that nearly all modern physicists and astronomers do not even as much as mention this proper motion of the spots as a possible factor in the causation of this, most perplexing, phenomenon. All kinds of inconceivable theories have been advanced in explanation of it.

Even the aid of the omnipotent and omnipresent contraction theory has been invoked—but with what success? Young agrees with Duner when the latter says: "I must confess that this difference between the rotation periods in the different latitudes appear to me incomprehensible and constitute one of the most difficult problems of astro-physics." "No theory," says Young himself, "yet presented is really satisfactory." And the proper motion of the spots has been entirely forgotten or neglected.

I must not omit, however, to point out that this may be, and probably is, owing to the fact that Duner thought that his spectroscopic observations on the velocity of the sun's rotation show the variation of periods to be inherent (?) in the photospheric substance itself. But, if one considers the great difficulty of these observations and the great incongruity of the results arrived at by different observers even as to the velocity of the rotation in a given latitude deduced from the spectroscopic observation—one can judge for himself what value is to be attached to these observations with reference to the equatorial acceleration. (See Note C at end of this section.)

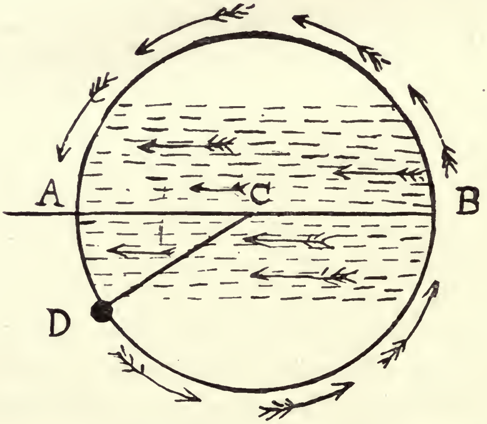
29. We, therefore, side with Rodwell's *Dictionary of Science* and attribute the phenomenon to the proper motion of the spots. But, in order that any proper motion of the spots should produce this phenomenon, it is necessary, first, that this motion should be regular, constant, and systematic; secondly, that the spot-motion should vary with the latitude. And the question at once arises, first, why should the spots move with reference to the photosphere with a systematic motion? Secondly, why should they move with different motions in different latitudes? And it is to these

questions that we claim that our theory offers a perfectly satisfactory solution.

There are two causes which produce motion of the spots, and both of them because the tendency of the expansive central forces is to direct the ejected matter in the direction of the motion previously existing in and around the ejected matter, as was explained in art. 11 on "Comets" (which we earnestly request the reader to reread). The first cause is the translatory motion of the sun around the Primosol, but the motion of the spots from this cause does not affect the rotation period of the sun deduced from the spots.

In the figure-below, let the circle, whose centre is marked C, represent a section of the sun perpendicular to the polar axis at any latitude. Let the line AB represent the direction of translatory motion of the sun in its orbit from B to A in the direction of the arrows within the circle.

FIG. 19



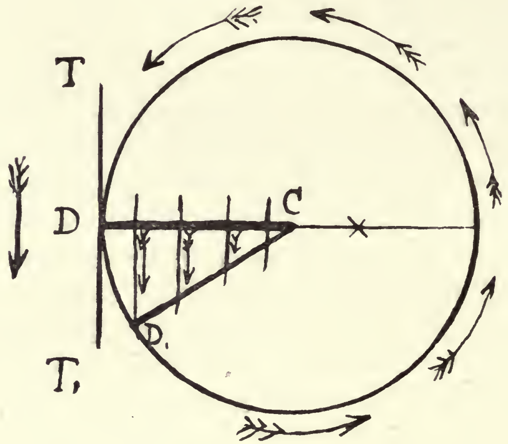
Imagine a spot to have originated at A, the anterior part of the sun (see art. 11 on "Comets"), and to have been carried away by the sun's rotation to D. Now, because all the particles of the sun's substance have a translatory motion in lines parallel to AB (as indicated by the dotted lines), therefore will there be a strong tendency in the matter ejected through the spot D to take the same direction; and the spot will, therefore, slowly move *backwards* nearer to A. This backward motion of the spot will continue with diminishing force through all the time that the spot is being carried by the sun's rotation from A to B. But after it passes B the proper motion of the spot due to the translatory motion of the sun will evidently be reversed and the spot will move *forward* nearer to A, so that in a complete revolution of the spot these two opposite motions will cancel each other, and the period of the sun's rotation deduced from the spot will not at all, or but slightly, be affected by the spot proper motion arising from this cause.

The second cause, giving rise to proper motion of the spots, is the rotary motion of the sun, and it is to the proper motion produced by this cause that the apparent equatorial acceleration is due.

Let figure 20 represent a section of the sun, the same as the former figure. Let D represent a spot; let the heavy line CD represent a column of matter which is being ejected by the deepest central forces. Draw the tangent T D T^1 and the small lines parallel to it across C D. These lines will indicate the direction of the rotatory motion, to which the matter to be ejected and that in its immediate vicinity is subject. Therefore, will the column of matter C D have a strong tendency to move in the direction indicated by the arrows; that is to say, it will continually strive to take the position C D_1 :(indicated by the light line), for C D_1

is more inclined in the direction of the parallel lines than is C D.

FIG. 20



Therefore, will the spot move continually and steadily through the photosphere from west to east, and the rotation period of the sun, deducted from this spot, will be smaller than it is in reality.

Now, since effects vary with their causes, this proper motion of the spot will vary with the speed of rotation to which ejected matter is subject; and, therefore, will it be smaller at greater distances than at smaller distances from the equator; and, therefore, will the period of the sun's rotation deduced from the spots appear longer at greater distances than at smaller distances from the equator—*equatorial acceleration.*

Let me say, in parentheses, that the simplicity, beauty, and perfection of this explanation of the otherwise highly perplexing phenomenon of the equatorial acceleration cannot fail to be convincing.

Let me also add that the spots have been actually observed to exhibit, at times, this strong tendency to move *forwards* on the disk of the sun.

"When any considerable change of form or structure shows itself in the principal spot of a group it seems to rush forward (westward) upon the solar surface, leaving its attendants trailing behind." (*The Sun.* Young, pp. 119.)

"Forward" from the solar west to the solar east, which is from our east to our west, as we look to the under surface of the sun.

30. We thus see, dear reader, how clear and transparents all the dark and greatly puzzling phenomena of our great luminary become by the light shed upon them by our theory. A light illuminating so many utterly dark regions, regions to the interior of which the human mind in its ceaseless travels could not penetrate for centuries, and this in spite of persistent and laborious efforts to do so; regions where the human mind has stumbled over and over again so many times because of the absolute darkness prevailing there—a light like this cannot but come from a great source—truth.

31. We are almost done with our subject, and we wish to emphasize that *our theory,* once accepted, as it must on the strength of our apriori reasoning expostulated in Book 4 and on the strength of the overwhelming "evidence" afforded to it by the Earth, by the planets, by the meteors, and especially by the comets—our theory once accepted and our inference with respect to the Primosol becomes almost a necessary inference.

For a sun to be a sun and to give out light and heat for centuries, for thousands—nay, for millions of years (for eternity, I might say)—it is absolutely necessary that it should move in a dynamical curve; that is to say, that it should revolve in an orbit around a center of gravity—a Primosol.

The fact alone that the sun rotates, makes it more than probable that it also revolves around a Primosol; for all the rotating bodies with which we are familiar —all of them also revolve around primaries.

The stars, when we come to treat of them in the next section, being, as they are, suns, will afford us overwhelming evidence of the existence of as many Primosols as there are shining orbs in the firmamnt, so that our solar Primosol is not unique and solitary, but one of a great, great family, which are nevertheless separate and have no connection one with the other.

Therefore it is that we consider our inference strong enough to withstand all and every objection that might be raised against it, even such that we have no idea of.

32. But the following objections were prominent in our mind even while framing our theory, and to them we will attempt to reply, as well as our ability will permit, trusting confidently that some, more able, will some day prove these replies to be true, or substitute others in their stead:

(a) If there be, as you say, such a Primosol within the insterstellar space allotted to the solar system—a Primosol that causes the large sun to revolve in a very large orbit within the short period of 11 years, and, consequently, with exceedingly great velocity—if there be such a Primosol, it ought to produce planetary perturbations, which we see not.

Our simple reply to this is that, owing to the inconceivably great distance of the Primosol from the sun (for we conceive it to be at least one thousand astronomical units), these perturbations are very minute and hard to be detected. And, moreover, as far as I know, there are even at present some outstanding phenomena in the motions of the planets and their satellites that have not been accounted for as yet on the Newtonian theory. Perhaps irregularities there may be that have been accounted for on other grounds than the true one (tides and equatorial protuberances), and our Primosol may, for all we know, be the true cause. (See Note D at end of this section.)

(b) If the sun be moving with such great velocity in such an exceedingly large orbit with a period of 11 years, this motion of the sun ought to be easily detectable in an opposite and apparent motion of the stars, which apparent motion ought to return upon itself in a period of 11 years. In other words, there ought to be easily noticeable parallactic motion of the stars due to the solar motion.

In reply to this, let me remind the reader that when the supposedly fixed earth was made by the great Polish philosopher, Copernicus, to move in an orbit with a radius of some 95,000,000 miles, the same objection was raised by the Ptolemeyans. They said, if the earth be moving in such a great orbit, we ought to see the earth's pole traveling from star to star in a circle in a period of one year. And the Copernicans could but give one answer, and that is, the great distance of the fixed stars from us.

Our solar orbit is many, many times larger than the earth's orbit, and, therefore, has this objection much greater force; but the answer must be the same, namely, that the fixed stars are at such vast distances from us that even this enormous solar orbit becomes, when compared with the star-distance, an insignificant quantity.

It will be said, and with reason: At the time of

Copernicus the distance of the stars was not known, but at present the heliocentric parallax of many stars has been determined, and if we multiply this parallax even by hundreds in order to get the primosolcentric parallax, we will get a very sensible quantity.

To this we reply, firstly, that the determination of the stellar parallaxes is such a difficult task, and the result arrived at by various authorities are so widely different, that the given parallaxes are not to be taken with positiveness as representing the real heliocentric parallax.

The following is a table of the various values of the heliocentric parallax of the *pole star,* as given by various authorities, and is copied from a large table by Newcomb in the *Popular Astronomy*:

Name of Star.	*Astronomer and Date.*	*Parallax.*
Pole Star.	Lindenau, from R. A's, 1750-1816.	0″.144
	W. Struve, Dorpat, 1818-'21.	0″.075
	Struve and Preuss, from R. A's, 1822-'38.	0″.172
	Lundahl, from Dorpat declinations.	0″.147
	Peters, from declinations, 1842-'44.	0″.067
	Lindhagen.	0″.025

It will be seen from this table that Stuve's and Preuss's determination of the parallax (0″.172) is nearly seven times larger than that given by Lindhagen (0″.025).

Of the parallax of Capella the table gives us two values—that of Peters, from declination, being (0″.046) and that of Sturve in 1885, with Pulkova equatorial, being (0″.305)!

Sirius's—ranges in the table from (0″.16 to (0″.273), and so on for every star in the table.

Secondly, we say that the displacement of the stars indicated by their various parallaxes is due to a double and composite displacement of the observer. On the one hand the observer is carried around the sun by the motion of the earth in her orbit, and on the other hand he is carried by the motion of the sun around the Primosol; and we say that the greatest part of the parallax may, for all we know, be due to the latter displacement. This circumstance will perhaps afford an explanation of the great discrepancy between the results of the observations on stellar parallaxes by various observers at various times. It is possible that the cause of these discrepances does not wholly lie in the difficulty, mode, skill and exactness of observation, but that it largely depends on the time of observation and the position of the sun in the orbit during the one year of observation. For, with reference to any star, there are certain parts of the sun's orbit through which the solar motion will produce a minimum of parallax, and certain other parts through which the solar motion will produce a maximum parallax.

(c) If the sun moves in a dynamical curve, the planets and comets necessarily move with it in the same curve, and must not, therefore, heat energy be developed in them because of this motion, in addition to that developed in them because of their systematic motion around the sun?

To this we say that the laws of the composition of systems of motion are not known. For all we know, the motion of the planets, together with the sun, around the Primosol may not affect their thermal condition, since the Primosol has so little perturbing effect on them, as was said under objection (a). We are inclined, however, to believe that the Primosol motion of the planets does affect their *thermal equivalent,* as was said in the general scholium to Book IV. (See Note F at end of this section.)

Additional Notes to "The Sun."

Shortly before going to press the author has purchased (October, 1903) the admirable work of Agnes M. Clerke ("Problems in Astrophysics") just published in London. A hasty scanning over of its pages has suggested to the author many valuable thoughts with reference to this work, among which are to be numbered the following notes. In fact, the book, in every page of it, is replete with strong corroborative evidence to "our theory":

Note A.

As bearing on the subject of the volcanic action involved in the process of sun-spot formation (see art. 10-11), the following two facts, new to the author, will afford additional testimony:

"An immense area of disturbance usually surrounds an active spot, and this area of disturbance is also an area of actual elevation. A series of micrometrical measures carried out by M. Sykora of the Charkow Observatory, in 1895, showed that, as a very general rule, the sun's diameter is lengthened in the direction of the spot on the limb. The fact is most significant, for it indicates relief of pressure as perhaps the cause, and certainly as an accompaniment of solar outbreaks, and thereby associates them with volcanic explosions." ("Problems in Astrophysics," p. 74.)

Again, on p. 85, we read: "A remarkable feature of photospheric commotion was referred to by Father Cortie at the meeting of the Royal Astronomical Society May 11, 1900. * * * A group of spots generally includes two chief members, posted respectively in the van and rear of the array. These *take it in turns* to develop. * * * Analogous pulsations, but on a larger scale, manifest themselves in responsive disturbances north and south of the solar equator. Mr. Maunder had already pointed out in 1894 that an "active train" of spots is often accomplished by a feebler copy of itself a few degrees north or south. An outbreak of the first magnitude," he continued, "will indeed reproduce itself in several directions." Somewhat similar correspondences are noticeable between volcanic foci on earth, yet the inference they suggest might prove misleading."

Note B.

In chapter 8 the author very ably discusses the spectrum of sun spots. One cannot be but struck by the great confusion of ideas prevailing in this department of spectroscopy, and by the numerous contradictory statements and assertions made by various men in attempting an explanation of the sun-spot spectrum; also by the number of insurmountable difficulties to be encountered by the theory of "cooled-off" gaseous absorption. "Still," the author says at the end of the chapter, "the coolest umbra must be hotter than the reversing layer, for otherwise the Fraunhofer lines would show bright against them. * * * This circumstance is fundamental in solar thermal relations, yet has generally been overlooked." (See above art. 13.)

Note C.

With reference to Duner's spectroscopic observations on the sun's equatorial acceleration, which tend to prove that the acceleration is not apparent, but real, and is inherent in the photosphere or at least in the reversing layer, the author properly expresses himself in terms implying doubt and uncertainty. "At the same time," he says, "it has to be borne in mind that Duner's observations rest upon a narrow basis" (p. 147). In fact, the great discrepancy between the period of rotation for any given latitude (especially the higher latitudes) as deduced from the motion of spots and facula on one hand and that deduced from Duner's observations on the other hand, highly discredits the reliability of the latter. So, for instance, the spot-period of rotation in latitude 15° is 25.44 days, the facular period for the same latitude is 25.26 days, while the spectroscopic period for the same is 27.42 days! Again, the spot period for latitude 80° is 25.87 days; that of faculae for the same latitude is 25.48 days, while that of Duner for the same latitude is 31.83 days! These figures speak for themselves and need not be dilated upon. (They are taken from a table in "Problems in Astrophysics," p. 146.) This note has reference to what was said above, in art. 28, with respect to Duner's observations.

Note D.

In art. 32, under (a), we said: " * * * there are even at present some outstanding phenomena in the motions of the planets and their satellites that have not been accounted for as yet on the Newtonian theory. Perhaps our primosol will afford the missing links." The following, from "Problems in Astrophysics," therefore, becomes highly interesting:

"One still more recondite and unaccountable fact has been lately adverted to by Mr. J. Halm. He contends that the physical condition of the sun reacts perceptibly on the motion of the earth. Variations in its orbital elements, which have hitherto baffled attempts at explanation on gravitational principles, are comprised, he finds, within the "great" spot period of about sixty-five years. The obliquity of the eclyptic, for instance, instead of decreasing uniformly with the time, shows subordinate fluctuations, synchronising with the long waves of solar activity." (By "long waves" the author means the great solar cycle of sixty-five years). "Its shorter waves" (11.1 years' cycle), "on the other hand, prove to be influential upon the variation of latitude. Mr. Halm considers that the deviations of the terestrial pole conform unmistakably to the eleven-year cycle, with, however, a lag of about one and a half years in the corresponding epochs. "It may," he writes, "be taken to be clearly established that the radius of the circle described by the pole of instantaneous rotation is greatest at times of sun-spot minima and smaller at times of maximum displays of solar spots" (pp. 158-159).

Now, does not this most significant fact of an eleven-year cycle of variations in terestrial latitude amount to an observational proof of solo-primosolic motion? Change of latitude is nothing else than a displacement of the stars, and is it not evident that this is caused by the eleven-year periodical motion of the sun? More, is it not the reason why the radius of the polar circle, mentioned above, is greater at spot-minima and smaller at spot-maxima—is not the reason for it, I say, because the solo-primosolic radius vector at aprimosole is greater than that at periprimisol, and therefore is the star-displacement greater at minima, when the sun is at a-primosol, than at maxima, when the sun is at periprimosol? I leave these questions to be decided upon by the candid, unbiased and penetrating mind of the reader himself?

Note E.

With reference to what was said in objection (c), art. 32, the following from "Problems in Astrophysics," is highly instructive.

Speaking of an hypothetical eleven-years' periodical change in the sun's electrical state, the author of the "Problems" says:

"The surmised alteration may be a consequence; it can hardly be the cause of solar periodicity. Its actual occurrence, however, is far from unlikely. Certain cometary phenomena lend it a qualified support. The grouping together near sun-spot maxima and minima, respectively, of the bright and faint apparitions of Enke's comet during a century (1786-1885) forced itself upon M. Berberich's attention in 1888. Correspondence of this unexpected form were even traceable with displaced epochs of activity, such as the retarded maximum of 1788, and the premature maximum of 1837. Some indications were besides gathered that comet discoveries become more numerous as the tide of solar energy rises, many that would otherwise pass unseen being lifted into visibility by access of transmitted excitement." (P. 158.)

The fact, then, is that comets give out more light and heat when their perihelion passage occurs at spot-maxima than when it occurs at spot-minima. This is the fact; but what is its interpretation? What is the cause of this most wonderful phenomena? The physicists are constrained to resort to a hypothetical "access of transmitted excitement" from the sun for an explanation. Our theory, on the other hand, offers the most perfect and most natural elucidation of the subject. At spot-maxima the solar system, including the comets, is at periprimosol, and is therefore moving with its greatest velocity. But a greater systematic velocity increases the energy value of the relative motion of one part of the system with respect to another. In other words, a greater systematic velocity increases the thermal equivalent for that system, and therefore is the amount of heat energy developed within the system (in any one of its parts) in consequence of relative motion in a dynamic curve greater, when the system moves with greater velocity, and less, when the system moves with less velocity. And, therefore, does Enke's comet appear bright when its perihelion passage occurs at sun-spot maxima—when the whole solar system moves with its greatest velocity; and again, it appears dark when it passes perihelion at spot-minima, because at minima the solar system is moving with its minimum velocity; and therefore has FVsna (the amount of hea- energy developed in it in a unit of time) its minimum value. (See and reread last scholium to propositions of Book 4; see also "Definition" in Book 2, section 3.)

SECTION VI.—THE STARS.

1. Even the stars, the beautifully twinkling stars; the stars, the grand, sacred symbols of eternity, truth and permanence—even these have been mercilessly attacked by the well-meaning physicists; for the physicists have unanimously and without much hesitation pronounced an irrevocable death sentence on these beautifully twinkling, and true to themselves, stars. Binary systems, triple systems, clusters, nebulae, the galaxy, all—all must go, all must die and cease to shine and twinkle, and thereby refute their own existence, thus belying themselves.

The spectroscope has proven the substance of the stars to be the same as that composing our sun and planets, and therefore must the death sentence, pronounced by the physicists on our solar system, be extended to all the stars—and no mercy given. Some of them may be *young*, some of them *old*, some of them in their *infancy*. Such are the nebulae. But they must go; they all must die, sooner or later. Die they must, and no mercy.

More, their only sin, the sin for which they must pay with their life and activity and become inert, dead and pulseless corpses, is nothing else but their great charity; their shining, their twinkling, their sending out blessings, life and activity to the needy in the infinite spaces surrounding them; and, among these, to the physicists and astronomers, their executors.

But the stars, the beautifully twinkling stars, laugh; "the sitters of the heaven laugh;" laugh at humanity and physicists, at their judgments and sentences. They laugh and continue their reckless charity, their squandering of blessings, undiminished; they continue to shine and twinkle with the same splendor. Sometimes, indeed, those of them that are called variable seem for a while to economize and diminish their splendor; but only for a while, and then they repay, and more than repay, what they have economized—and seem to tease us and laugh at us.

How ill accords the dictum of science that the stars, like the sun, must sooner or later grow dark and cold, mere relics of bygone activity, with a view of a clear and serene sky in a moonless night. No sign of decay, of decline, or dissolution, are anywhere to be traceable nor found. For more than 3,000 years have the skies been assiduously watched, the stars counted, their situations noted and appearance described, and we are in the possession of these records—and still there is not a single star to which we can point and say: "There are signs of dissolution and decline in the present appearance of this star, when compared with its appearance 3,000 years ago."

2. The arguments against this dictum of science with reference to the sun, and the arguments against the meteoric and contraction theories, under cover of which the physicists attempt to hide themselves—these arguments have been sufficiently discussed in articles 2-8 of the preceding section and also in the section on "Earth." These arguments are with equal—nay, with greater—force applicable to the stars also. For the stars are suns and the sun is neither more nor less than a star!

Therefore, if, for a sun to be a sun, it is absolutely necessary that it should move in a dynamical curve, then, a fortiorily:

For a start to be a star, it is absolutely necessary that it should move in a dynamical curve around a centre of gravity—a Primosol.

And in this section it is our intention to show how this statement is borne out by the stars generally. Generally, for were we to examine the stars in detail, a large volume of many pages of very interesting matter might be filled—an undertaking for which we are not ready at present.

3. First of all, then, our assertion implies a necessity for inherent motion in each and every and all stars as an essential perequisite to be stars—shining orbs. For, in accordance with our theory, two causes conspire and are essential to make a star—a star, or a sun—a sun. The first is the efficient cause—the gravitational attractive force of a body external to the star—of a Primosol. The second is the occasioning cause—the inherent motion of the star itself. Annihilate this motion and the efficient cause—gravitation—cannot act, and the star ceases to be a star—a shining orb.

Therefore, our assertion implies that the "fixed stars" *must* move individually and each for itself independently of the other.

How is this statement borne out by the skies? For generations the stars have been considered fixed and absolutely at rest. But with the great improvements the telescope has undergone in the last few decades, and with the introduction of the spectroscope to indicate and measure motions of far distant luminous objects, it was soon discovered "that among the tens of thousands of stars which fill the heavens not a solitary one, in all probability, is in a state of absolute rest.

" * * * Motion, ceaseless, eternal motion, seems to be stamped on the entire universe." (*Planetary and Steller Worlds Mitchell Lectures.*)

"We may assume that the stars are all in motion. It is true that only a comparatively small number of stars have been actually seen to be in motion; but as some motion exists in nearly every case where observations would permit of its being determined, we may assume the rule to be universal." (*The Stars.*. Newcomb. Ch. 6.)

This motion of the stars has been found to be in different directions and with different velocities; that is to say, each star was found to move individually and independently of the others, with a velocity and direction of its own.

Why, I ask, this universal, ceaseless, eternal motion? Is it a mere, aimless, purposeless and wandering motion? Is it a mere whim and freak of a capricious Nature? Or can you attribute this individual and independent motion to the effect of the gravitation of the stars mutually one upon the other? Is it not as clear as daylight that the stars move, because for a star to be a star it *must* move; because without motion a star —a shining orb—cannot be what it is—a star. For my part it is; and before the reader leaves this section he will also be convinced that it is, so I hope.

Further, if we are correct, and if motion is essential to the being of a star, then a star with greater motion must, other things being considered equal, be a greater star—a brighter star, I mean. For a greater motion in a star implies a greater gravitational force controlling that motion, and it is upon the magnitude of this force and the velocity of the motion that the amount of heat energy developed depends. Now, what is the actual condition of things?

"The general rule is that brighter stars have the largest proper motion." (*The Stars,* pp. 77.)

The astronomers' explanation of this is that the stars *appear* brighter because they are nearer, and therefore does their proper motion also *appear* larger. This is undoubtedly true to some extent, but positively not to the whole extent, as is very well known to any one who is acquainted with the subject.

And, I say, that our theory will some day contribute greatly to help clear off the intricacy of the relations of magnitude, parallax, and proper motion, which are very poorly understood at present. The subject is so extensive and so entangled, and the data upon which a solution might rest are so meagre, that we do not dare to enter into any dismussion of it, especially since what we call proper motion is in reality constituted, according to present conceptions, of three motions: (1) The apparent or paralactic motion; (2) the proper motion, properly so-called; (3) the orbital motion of the star around its Primosol, as will be rendered evident subsequently.

I wish, however, to point out that, in accordance with our theory, the magnitude (brightness) of a star should depend on, and be proportional to, the following factors:—

(a) The nearness of the star to us.

(b) The velocity of its orbital motion.

(c) Its mass (see Art. 15 on "Earth").

(d) The nearness of the star to its Primosol, as is evident from formula xiv in corollary to proposition 8, in Book IV

With the increase of each of these factors the brightness of the star will also increase. This, however, holds good in the case of factors (b), (c), (d) only up to a certain limit; and the further increase of these factors beyond that limit will entail a diminution of the star's brightness. (See Art. 15-16 on "Comets." Also Art. 12 on the "Sun.")

4. In the second place, our statement implies that for a star to be a star there must need be a body external to the star, exerting gravitational force on it, and constantly deflecting it from the straight line of its inertial motion. Our statement implies, in other words, that for the being of a star—a shining orb—a duality is absolutely essential; a duality—an existence of *two* bodies gravitationally connected one with the others; the one a star—a sun; the other a centre of gravitation for that sun—a centripetal centre—a Primosol; the one—the Primosol—exerting gravitational energy, the other being exerted upon—receiving, so to speak, that energy and manifesting it in light, heat, and actinic rays—in radiance.

Therefore, if our theory be accepted as a positive truth,—*wherever there exists a permanently radiating body—a star—there must, necessarily, exist another, a second, body, gravitationally connected with it*; *the two revolving around their common centre of gravity and constituting a "binary system."*. And the existence of a single star: a star unconnected gravitationally with another and not deflected from the straight line of its motion by the gravitational energy of that other—is an absolute impossibility.

Therefore, all the stars we see with the naked eye; all the stars we perceive by the aid of the telescope; even all those that shine at such vast distances from us that their light does not reach us at all—all are, each and everyone of them without exception, a binary system.

5. Now, what do the heavens say to this? What do the stars, themselves, declare? I say, that they—the stars, themselves—in most positive and unmistakable language confirm our statement. When Sir

William Hershel first turned his greatly improved telescope toward the starry heavens, he was greatly astonished to find that some of the stars, which to the naked eye appear single, really consist of a *pair* of stars in very close proximity one to another. Reasoning and observation convinced him and the astronomers after him that most of these double stars are not merely optically double nor are they accidentally double, but that they are really and physically double; that is to say, that, with the exception of a very few, all the pairs of all the double stars are united, one to the other, by a physical bond—they constitute a system: a binary system. Further observation has proven, in the case of a great number of double stars, that the components of each pair actually and really revolve one around the other, or, more exactly, around their common centre of gravity; and to such the term "binary system" has been applied. And at present no shadow of a doubt is left in the minds of all astronomers that all the double stars revolve, each pair for itself, around their common centre of gravity; in other words, that all the double stars, whose number at present is to be counted by tens of thousands, are really so many binary systems.

"There can be no reasonable doubt that the two components of all physically connected double stars revolve around each other. If they did not their mutual gravitation would bring them together and fuse them into a single mass. We are, therefore, justified in consideding all double stars as binary system, except those which are merely optically double. For reasons already set forth, the pairs of the latter class which are near together must be very few in number." (*The Stars.* Newcomb, pp. 157-158.)

6. Now, it is to be asked: What is the meaning of all this? Why this multiple duplicity of the stars? Why should the number ("2") be so prevalent in the heavens? True, there are some triple, quadruple systems and clusters of stars. But, firstly, the number of these vanishes into insignificance when compared with the number of double stars, especially when account is taken of the spectroscopic binaries to be mentioned hereafter, which bring up the number of binaries to millions. Secondly, many stars of the so-called clusters have been found to be binaries. In fact, the number of binaries within clusters is greater than in other situations (as to be infered from the great number of variable stars they contain.) (See "The Stars," pp. 173-174.)

"That such condensed clusters should have a great number of close binary systems is natural, almost unavoidable we might suppose. It is probable that among the stars, in general, single stars are the exception rather than the rule. If such be the case, the rule should hold yet more strongly among the stars of a condensed cluster." (*The Stars,* pp. 177.)

We thus see that number "2" plays the role of an element in the starry heavens of which larger compounds (clusters) are compounded, if a cluster be considered to represent a system of stars in some way physically connected.

It is greatly to be wondered at, how it happens that the astronomers and physicists accept this apparent duplicity of the heavens as a matter of fact without further comment, and do not seem to trouble themselves at all about an explanation of this most startling fact. They call the *lesser* component of a pair "companion," "satellite," or "planet," and are satisfied. Why should all, or nearly all, or, at least, the majority of the stars have a companion? Why only one companion? Why a companion vying with themselves in mass, and, in a great many of the telescopic binaries, even surpassing them in the intensity of its light, as will be pointed out further on? To these and many other inexplicable facts connected with this duplicity, the physicists give no answer, and satisfy themselves with saying that those things are rather curious.

Is it not, dear reader, as clear and transparent as daylight that this multiple duplicity, this general prevalence of "twos,"—of pairs—in the starry heavens must have a cause; and that since this duplicity is *universal,* the cause must likewise be *universal?* But a universal cause must be a necessary cause, an essential cause; and this means that the stars are double because they cannot be otherwise than double: because duplicity is essential to the very being of a star; in short, because *for a star to be a star it is absolutely necessary that it should move in a dynamic curve around a centre of gravity—a Primosol.*

We said that the star duplicity is universal, for such is the unavoidable inference to be drawn from the great number of the recently discovered spectroscopic binaries to be dealt with hereafter. Professor Newcomb says "single stars are the exception"; but we may with great positiveness say, single stars are an utter impossibility and a contradiction to Nature and her laws.

7. Before we proceed to examine certain facts connected with telescopic binaries and in anticipation of facts to be brought forward in connection with spectroscopic binaries, we wish to lay down some principles with reference to the relation of the components of a binary, one to another.

(A) The greatest difference between the two components, on which all other differences depend, is constituted by the difference of their respective masses. This is evident. For, if their masses be equal, their velocities and the centripetal forces by which each is acted upon are also equal, and therefore must the amount of energy generated in each of them be equal to that of the other, and therefore, they must be of the same magnitude, of the same color, and of the same everything: density, dimensions, etc. There may be, though, a difference of chemical substance, which may give rise to other differences; but since the substance of all the stars is considered and has been proven by the spectroscope to be almost identical, we may neglect this.

Now, as of the almost infinite possible proportions of the masses of the two components, equality is only one; therefore, we should expect to find in the majority of binaries a great number of differences between the two components, as that of orbital velocity, magnitude, color, density, dimension, etc.

Remark. The component with the greater mass we will term Primosol, and the component with the smaller mass—sun, or star.

(B) Other things being equal, the greater the conjoint mass of the binary the greater the amount of energy developed in, and disposed of by, the system.

(C) Other thing being equal, the less the distance between the components the greater the quantity of energy developed in, and disposed of by, them. (Formula XIV, Prop. XIII, Book IV.)

(D) The amount of energy developed in a unit of time in the larger component—the Primosol—is to that developed in the lesser component inversely proportional to their masses. Let M and M^1 be the masses of the larger and smaller components respectively; let E and E^1 be the respective amounts of energy, then

$$\frac{E'}{E}=\frac{M}{M'}$$

that is to say, the amount of energy of the lesser will be greater than that of the larger component. For, let F and F^1 be their respective gravitational intensities, then will the amount of energy developed in a unit of time in the Primosol be equal to MF^1V, and that of the star to M^1FV^1, where V and V^1 represent their respective velocities (the orbits of the two around their common centre of gravity being supposed to be circular. See Prop. XIV, Book IV). We will therefore have the following two equations:—

$$E=MF^1V \quad (1)$$

$$E^1=M^1FV^1 \quad (2)$$

Divide equation (2) by equation (1), and we get

$$\frac{E'}{E}=\frac{M'\,F\,V'}{M\,F'\,V}=\left(\frac{M\ \ F}{M\,F'}\right)\left(\frac{V'}{V}\right)\ \cdots\ (3)$$

But $\left(\frac{M'\,F}{M\,F'}\right)$ is equal to unity, for $\frac{F}{F'}=\frac{M}{M^1}$ the gravitational intensity of a body being proportional to its mass. Therefore equation (3) becomes $\frac{E'}{E}=\frac{V'}{V}$

But $\frac{V}{V}$ is equal $\frac{M'}{M^1}$; for the diameter of the orbit of the lesser is to that of the larger component as (M) is to (M^1); and since both components have the same period, therefore must the velocity of the lesser component be to that of the larger, as M is to M^1.

Substituting in equation (4) $\frac{M}{M'}$ instead $\frac{V^1}{V}$ we get, finally, $\frac{E'}{E}=\frac{M}{M'}\ \cdots\ (5)$

and from this last equation we see that E^1, the absolute amount of energy developed in a unit of time in the lesser component, is larger than that developed in the same time in the larger, for $M > M^1$.

(E) The magnitude (brightness, visibility) of a star depends not on the absolute amount of energy radiated by it, but, firstly, on the character of its rays, whether within the light scale or not. For a star may really radiate a large quantity of energy because of its great mass, and still be of low visibility, or entirely invisible, because its rays are mostly or wholly below the red—heat rays; or a star may realy emit an exceedingly large amount of radiance because it is intensely and exceedingly "hot," and still be of low or of no visibility, because on account of its intense heat its rays are mostly or wholly those above the violet—actinic rays. (See Art. 12 on "Sun." Also, Arts. 15-16 on "Comets.") Secondly, magnitudes depend on the extent of the radiating surface, as is self-evident.

8. It will be rendered evident from the above five principles that the visibility and brightness (magnitude) of each and of both components of a binary depend on the following three factors:—

(a) Their absolute masses (Principle B).

(b) Their distance from each other (Principles C).

(c) The proportion the two masses bear to each other (Principle D). And as these vary, the magnitudes of each and of both components vary. From the infinite number of possible variations in the combination of these three factors and the peculiar feature of the binaries depending on these variations, we will point out the following typical variations:—

Type I. The masses of both components may be so small, or their distance so large, that the amount of heat-energy developed in them is not sufficient to cause incandescence in neither of them, and both of them will be invisible. Whether binaries of this type actually exist in nature, we have, of course, no means to ascertain.

Type II. The masses of both components may be so large and their distance from each other so small, that the amount of heat-energy developed in either of them will be so exceedingly great, that the rays emitted by either of them will be those above the violet; and both of them will, therefore, be invisible.

Type III. Moderate masses, moderate distances, and a proportion between the masses not very far from unity, will cause visibility of both stars. To this type belong all the telescopic binaries. As the amount of heat-energy developed in the smaller component will be larger than that of the larger (Principle D), and as this larger amount of heat will have to be radiated away from the smaller compopnent by a surface smaller than that of the larger, it follows that the absolute temperature of the smaller component will exceed that of the larger; and therefore will the color of the smaller be different from that of the larger. And as the difference will be due to a greater amount of rays of high refrangibility in the smaller, therefore, will the color of the smaller be more intense than that of the larger, so that the larger may be reddish or yellowish, while the smaller is white, greenish, or bluish, etc.

Type IV. Reduce the mass of the smaller component in Type III; reduce also its distance from the Primosol, and the intensity of its light will become so great, that it, the smaller component, will become entirely invisible, and we will see the larger one alone. To this type belong some of the spectroscopic binaries, so I believe.

Type V. M (principle D) may be so large and M[1] so comparatively small and the distance so considerable that E will not be sufficient to render the Primosol visible; and what we see will be only the smalled component. To this class belong most of the spectroscopic binaries, so I believe. And this is, I believe, the relation *our Sun* bears to its Primosol.

Type VI. The disproportion between M and M[1] may be so great that while the Primosol remains invisible because of deficiency of heat-energy, the heat-energy of the sun will become so excessive that the sun will become nebulous. This is my explanation of nebulæ, of which further on.

Type VII. Carry Type VI to its extreme and the nebula becomes entirely invisible; so that we have a binary in which the larger component is invisible because of deficiency of heat, while the smaller is invisible because of excess of heat.

9. The principles in Art. 7, and the types of binaries in Art. 8 are merely statements of what ought to be if our theory be correct; our theory given and they must follow. Therefore, if facts discovered by observation should happen to be in perfect harmony with these apriorily deduced principles—this will constitute conclusive evidence for the truth of our theory. But the facts discovered by observation *are* in perfect harmony with these principles, as will be rendered evident

as we proceed with the rest of this section. At present we wish to point out the following simple facts presented by telescopic binaries:

(a) In a list of the more remarkable double stars given by Professor Nemcomb in his *Popular Astronomy,* out of 78 stars in the list there are only nine doubles, the magnitudes of the two components of which are the same; and this is in perfect harmony with what was said in Principle (A), Art. 7.

(b) By our principles, the same magnitudes imply the same masses; and, therefore, there must be no difference in color between the components of a binary whose components are of the same magnitude. And such is really the case, as will presently be pointed out.

(c) By Principle (D) and Type III (Art. 8), the color of the smaller component must be more intense than that of the larger component. And such is, in reality, the case.

"In a large proportion of cases, perhaps, one-third of all" (?), says Professor Young, "the two components are very nearly equal; but in many they are very unequal. In this case (never when they are equal) they often present a contrast of color, and when they do, the smaller star, for some reason not yet known, *always* has a tint *higher in the spectrum* than that of the larger: if the larger is reddish or yellow, the small star will be green, blue, or purple." (Young's *Elements of Astronomy,* Art. 461.)

(Mark especially the phrases: *"never when they are equal"* and *"for some reason not yet known, always,* etc."

Yes, our theory offers the most satisfactory and most logical reason for it: it shows that it *must* be so; that a tint lower in the spectrum for the smaller component is contradictory to the laws of nature—"*always!*" yes, always and forever!

10. From what preceded we see that our theory forces upon us the conclusions: firstly, that our sun is a binary of the 5th type, the larger component of which is dark and cold, because of its great mass, as compared with that of the sun (Principle D); secondly, that each and every star we perceive as single through our strongest telescopes is, in reality, a binary, either of Type IV or of Type V; that is to say, either the smaller component is invisible because of its excessive heat, or the larger component is invisible because of deficiency of heat-energy. Now, should observation confirm this and prove to us that *some* single stars *are* really binaries whose second compenents are invisible because of light deficiency, then, we will, by the law of uniformity of nature, be compelled to infer that all single stars are such binaries; and our theory will thereby receive the most positive and decisive evidence. But observation does prove some single stars to be such binaries.

Aside from the inference to be drawn from variable periodic stars, the late researches of astronomers, and especially the researches of Professor Campbell, Director of the Lick Observatory, have demonstrated in a most positive manner, almost amounting to occular demonstration—that many, many, indeed, single stars are, as they say, attended by a dark and invisible companion; that is to say, that they are really binaries, one component of which is invisible because of light-deficiency.

The instrument, through which this grand and most potent discovery was made, is the spectroscope. It is very well known that in the spectrum of a luminous body, moving directly toward us in the line of sight, the Fraunhofer lines, dark or bright, get displaced from their normal position toward the violet end of the spectrum; and the amount of this displacement bears a definite relation to the velocity of the body in the line of sight. On the other hand, if the luminous body is moving *away* directly from us, the lines are displaced toward the red end of the spectrum; and the amount of this displacement will constitute the measure of the velocity with which the body is moving away directly from us in the line of sight.

Now, suppose in the spectrum of a star a displacement of the lines toward the violet to have been found; and suppose that night after night, while watching this spectrum, we notice a gradual diminution in the amount of this displacement: the lines gradually approaching their normal stationary position; and suppose that the lines, after reaching normal position, have not stopped their journey toward the red end of the spectrum, but moved onward up to a certain limit; and that then they commenced to move back toward the violet, and finally regained the position from which we started, only to recommence the same journey toward the red again—would not all these observations amount to almost an occular demonstration, that this particular star is moving in an orbit whose plane is either in the line of sight or inclined to it in an angle less (or more) than 90 degrees? And that, therefore, does the star, when in one part of the orbit, move away from us, and, when in the opposite part, move toward us?

This is precisely what Professor Campbell and some others have done and have observed; and so it was found that many, many stars move in closed orbits. But orbital motion implies a centripetal centre, an attracting mass—a dark and invisible component.

In a communication to "Science," published on the

17th of April, 1903, Professor Campbell says: "It is apparent that of the brighter stars, at least one in every seven or eight, is attended by an invisible companion.

"When we consider that spectroscopic methods are at present capable of discovering only the larger variations, that very few stars of long periods have probably been advantageously observed as yet, and that the velocity of our sun, due to the orbital motions of the planets attending it, has a double amplitude of only two or three hundredths of a mile per second, there can be no doubt that the number of the spectroscopic binaries must be very great. It is probable that the star unattended by dark companions will be found to be the exception rather than the rule."

In response to a direct inquiry, Professor Campbell was kind enough to write to me: "I may say that my spectroscopic observations have established that at least one star in seven, though appearing single in telescopes of the highest power, has one (or more) massive dark companion revolving about it."

And, let me say right here, that Professor Campbell deserves the deepest gratitude of mankind in general, and of scientists in particular, for his indefatigable labor in this particular branch of astronomy; for his spectroscopic binaries are pregnant with great meaning: meaning not at present suspected, even by himself, but which will gradually grow more and more apparent as the years roll on.

We, in particular, owe great gratitude to this great man, for his spectroscopic binaries have rendered us the most inestimable service; they afford an almost occular demonstration of the truth of our theory. Through pure reasoning and on the strength of a single law of nature, we apriorily concluded, some four years ago, that for every single star in the heavens there must be another; a dark and invisible star around which it revolves. Such an apparently absurd notion had, of course, to be harbored in silence with the greatest secrecy. And here comes Professor Campbell and says openly: "I saw your dark bodies through the eye of the spectroscope, and can see one such for every luminous orb in the heavens." What a service. and what a gratitude do we owe him!

Be it remarked, in parenthesis, that our information with respect to astronomical matters being derived mainly from text-books not very recently published, Campbell's spectroscopic binaries became known to us but a short time ago.

11. The following quotations from *The Stars,* by Professor Newcomb, cannot fail to be of interest in this connection:—

"The great revelations of our times have come through the application of this method of measurement of motion in the line of sight from us to a star. No achievement of the intellect of man would have seemed farther without the range of possibility to the thinker of half a century ago than the discovery of invisible bodies which are now being made by such measurements. . . . The dark bodies, planets we may call them, which are revolving around the stars, must be forever invisible in any telescope that it would be possible to construct." (Page 11.)

Again, he says: "Applying the method to the variable star Algol, he (Vogel) proved that the loss of light which it undergoes at intervals of nearly three days is merely a partial eclipse by a dark planet, almost as large as itself, revolving round it. Thus was discovered a new order of bodies in the universe, telescopic binary systems, pairs of stars, or stars and planets, revolving round each other by their mutual gravitation, . . ." (Page 12.)

Further, he says: "It was Campbell who, with this instrument, has inaugurated a series of discoveries in this line which are without a parallel. He finds that about one star in thirteen has a planet revolving round it, so massive as to change the motion of the star by an amount visible in the spectroscope. . . . The final conclusion from all his work is that isolated stars may be the exception rather than the rule." (Pp. 12-13.)

As *The Stars* was published in 1902, it is seen that in somewhat more than one year, Campbell has succeeded in doubling the proportion of spectroscopic binaries from one in thirteen to one in seven; and this fact will tend to prove the generality of such systems.

12. Sometimes the second component, though invisible through the telescope, becomes actually visible through the spectroscope: the two stars forming two distinct spectra, one superimposed upon the other; and, as the two components, while revolving around their common centre of gravity, must move in opposite directions, therefore, it happens, when the line joining the two components is perpendicular to the line of sight, that the two sets of Fraunhofer's lines are displaced in opposite directions: the one toward the violet and the other toward the red; more, the two sets of lines have a relative periodic motion one with reference to the other.

The case of Capella is very striking in this connection. It first became known as a binary, through its double spectrum, by Campbell and Newall, independently, in 1900. Even its period of revolution has been spectroscopically determined to be 104 days!

(See "The Stars," p. 168.)

13. The concurrence of opinion of all astronomers is that these invisible components, "planets," as they call them, are of very enormous mass, not only when compared with our own planets, even with the largest of them—Jupiter—but also when compared with their own sun; for most of them were found to vie in mass with their sun and some even to surpass it. These conclusions have been forced upon the astronomers from a consideration of the spectroscopic motion and brightness of the visible components. And the astronomers wonder at this great mass of the star-planets, being, as it is, out of all analogy with what we know to be the case in our solar system. They wonder and pass this fact in silence, only remarking that it is rather curious. But this wonder and curiosity at once vanishes by the application of our theory. For we believe, nay, we are convinced that most of the dark components must exceed in mass many times their bright companions, for we believe most of the binaries to be of Type V: the type to which our own sun belongs; and in this type the dark component *must* greatly exceed in mass its brighter companion. For, for a component to be invisible because of light-deficiency, its mass must either be exceedingly greater (Type V), or exceedingly smaller (Type IV) than that of its brighter companion; for with nearly equal masses there must necesarily go nearly equal brightness. Therefore are we convinced that most dark components not only vie in mass with their brighter companions but must necessarily surpass them in mass, many, many times. For, be it remarked, that the statement of astronomers that the masses of the invisibles are, in most cases, not far from that of their brighter companions, is not to be

taken as a fact established from observation. It may only be looked upon as a guess. The only thing they can say with positiveness is, that the mass of the invisibles is very great, but what relation it bears to the mass of visibles there is no positive way of knowing. It should not be forgotten, that the astronomers are biassed in their opinion toward making the dark *planet* smaller in mass than its sun; and, therefore, it is that they generally say: this or that star has a dark body revolving *around* it, instead of saying this or that star revolves around a dark body, which would be the more proper and truer expression; for the motion detected by the spectroscope is not of the dark but of the bright body.

14. The case of Beta Lyræ is strikingly instructive in this connection.

Beta Lyræ is a variable star with two unequal minima in a period of 13 days, and the astronomers were unable to account for this double minimum variation of the star, until Pickering found a doubling of lines in the spectrum (see Art. 10). Thus it became evident to Professor Myers, of Indiana, that Beta Lyræ consists of two stars of unequal brightness, one obscuring the other when their line of junction coincides with the line of sight; and, therefore, the double and unequal minimum; for at one time, during a complete revolution, (A) obscures (B), and at the other, (B) obscures (A); and if (A) be brighter than (B), the minimum when (B) obscures (A) will be the larger minimum, and the minimum when (A) obscures (B)—the smaller minimum. What concerns us here, however, is Myer's conclusion that it is the star with the smaller mass that is the brighter of the two.

"The larger body is about 0.4 as bright as the smaller." (*The Stars,* p. 107.)

And so we see here that the darker body exceeds in mass the brighter one, in perfect accordance with our theory. It is also very interesting to note that "the mass of the larger body is about twice that of the smaller." (*The Stars*). See Principle (D).

15. Let me remark right here that this large mass of the invisibles deals the severest death—blow to the three theories purporting to explain the sourcess of the sun's heat supply, namely: (1) the meteoric; (2) the nebular hypothesis; (3) the contraction theory of Helmholtz. For neither of them can account for the invisibility of a body as large as one visible. The same number of meteors must be supposed to rain on both of them. Neither can it be seen why in their development from the same nebula one should have succeeded in cooling off sooner than its companion, since they are both of the same mass, and sometimes the cooled-off one is even the larger in mass. Neither can it be seen why one's constraction should have given out earlier than that of the other.

16. The subject of periodically variable stars is very important for us. For as our theory gives a most complete and satisfacttory explanation of these wonderful and unaccountable changes, it thereby itself gets the strongest possible confirmation from the starry heavens.

Periodicaly varying stars have been aptly divided by Pickering into two classes.

(a) Stars, the variability of which is caused by the interposition of a second component between them and us. The second component may be either dark as is the case with the variable Algol, or bright, as is the case with Beta Lyræ mentioned in Art. 14. In this class "there is no real variation either in the constitution of the star or in the actual amount of light which it emits." (*The Stars,* p. 116.)

(b) "The second comprises stars in which the changes of light are real, and arise from some cycle of changes going on in the star, but which may be due to the action of an external body." (*The Stars,* p. 117.)

17. Although we have no direct concern with the variables of the first class, still, in order better to appreciate the truth of our remarks with respect to the second class, we will here describe the variable star Algol, which is taken as a type for many other stars varying like it.

Algot is most of the time of about the second magnitude, but at intervals of somewhat less than three days it, somewhat abruptly, fades away and becomes of about the fourth magnitude, in which condition it remains for a few hours, and then as abruptly regains its former light and becomes again of the second magnitude, to fade away again in another three days. Now, even before the days of the spectroscope, it was suspected that this periodic sudden diminution of Algol's light may be due to a partial eclipse by a dark body revolving around it. The application of the spectroscope to Algot converted this mere hypothetical supposition into a certainty; for, Vogel, of Potsdam, has proven by the spectroscope that one-fourth of the time of a complete period (about 17 hours) *after* the minimum Algot is moving toward us; and 17 hours before minimum Algot is moving away from us.

Thus the revolution of Algol around a dark centre has been positively proven; and also that at minimum the dark body is in the line of sight, between us and Algol.

I purposely said that Algol was revolving around the dark body and not that the dark body was revolving around Algol, for in accordance with our theory, the dark body, because it is dark, must be at comparative rest and of a mass exceeding that of Algol, many times. And this is more befitting our case; for, considering the short period of Algol and its consequent great velocity, it is more natural to suppose that its motion is governed by a mass greater than itself; besides, strickly speaking, it is the motion of Algol that has been proven and not the motion of the dark body, and we have absolutely no right to put that body in motion.

Beta Lyræ, another type of the first class, has already been described.

18. We will subdivide the second class of periodic variables into two sub-classes:

(a) Those with longer periods.
(b) Those with short periods.

Omnicor Ceti, whose period is 332 days, nearly, is a good example of the first.

Eta Aquillæ, whose period is a little more than seven days (7.167 da.) is a good example of the second.

In both of these stars, as is indeed the case in all of the second class, the changes of light take place gradually and not abruptly (as is the case with Algol); so that between maximum and minimum and between minimum and maximum there are no two instants of time in which the star is of the same magnitude.

Further, in both of these stars, as is the case with all variables of this class, the change from minimum to maximum is rapid, while the change from maximum to minimum is slow.

"In most cases they (the variable stars) increase rapidly in brightness during a few days or weeks and then slowly fade away." (*The Stars*, p. 96.)

Again: "As is with other stars of this kind, it (Omnicorn Ceti) brightens up more rapidly than it fades away." (*The Stars*, p. 100.)

Omnicorn Ceti is visible to the naked eye only for two or three months, when it is at its brightest; for the balance of the time it is invisible to the naked eye, being then of the eighth or ninth magnitude only. It can, of course, be seen through the telescope.

The spectroscopic observations of the radial motions of Eta Aquillæ are peculiarly interesting to us. It was discovered by the spectroscope that the time of maximum brightness of the star nearly coincides with the time of its greatest velocity to us (16 kilometers); and that the time of minimum coincides with the time of its greatest velocity from us (some 24 kilometers). From which facts it is plainly seen that (1) Eta Aquillæ revolves around a dark body as a centre; that (2) its orbit is eccentric, that (3) the plane of the orbit passes near the solar system, or is in the line of sight; that (4) the line of sight intersects the orbit transversely, that is to say, it is at right angles (or nearly so) with the longer axis of the orbit; that (5) at or near periprimosol, the star is at its minimum brightness; and that (6) at or near a-primosol it is at its maximum brightness.

In passing, let me remark that the orbit of Delti Cephei, a variable of the second class, is also eccentric, and that as far as I know all the orbits of this class of variables are eccentric.

19. Now, what is the meaning of all this?

(a) To what cycle of changes going on in the star are these variations due?

(b) Why are the variations gradual and continuous?

(c) Why does the period of those variations correspond exactly to the period of revolution of the star around its Primosol?

(d) Why are the orbits of these stars eccentric?

(e) Why do they increase in brightness more rapidly than they fade away?

(f) Why do the maximum and minimum have such intimate relations with the periprimosol and a-primosol, as was pointed out in the case of Eta Aquillæ?

We claim that our theory at once and most satisfactorily answers all these questions.

20. According to our theory the heat-energy developed in a star, and, consequently, its brightness, depends on the velocity of its revolution and on its nearness to its Primosol. Therefore, in an eccentric orbit, the amount of heat-energy developed at the periprimosol will be the greatest, and that developed at the aprimosol will be the smallest; and, therefore, will the star vary as it approaches or recedes from the Primosol; and the greatest difference in brightness will be between that when at periprimosol and that when at a-primosol.

Two cases have to be distinguished: First, when the average temperature is medium. In this case the star will be brighter at periprimosol and dimmer at a-primosol. Second, when the average temperature of the star is excessive: near the limit of invisibility. In this case, the star will be brighter when it cools off—at aprimosol—and dimmer when it gets more heated—at periprimosol.

To the first class belong, as they ought, the variables of longer period: Omnicoru Ceti as a type.

To the second belong, as they ought, the variables of short period: Eta Aquillæ as a type; and, therefore, is Eta Aquillæ brighter at a-primosol, as was pointed out in Art. 18.

Our theory, then, at once answers all the six questions enumerated in Art. 19. For particulars with respect to these questions, see our explanations of similar questions on "Comets"; for these variables, moving, as they do, in eccentric orbits, behave exactly like comets, which also move in very eccentric orbits.

It may be objected and said, how is it possible that a star—a sun—should, because of a diminution of its heat-energy supply, lose so much of its light in a comparatively very short time (a few months, in the case of Omnicoru Ceti)? To this we will reply that although we have calculated elsewhere that the store of heat within the sun would give out only in 5,000 years, still there can be no doubt that were the sun to cease revolving around its Primosol, he would become perceptibly dimmer within the short period of one year or less. And our sun, we believe, is very large, when compared with the mass of the variables.

The stars, like the comets, then, loudly proclaim the verity of our theory.

We should have liked to say a few words about new stars and about non-periodic variables, but as this would greatly extend the limit of this section, we left these things out. In fact, a full exposition of our theory, as manifested by the stars, requires a separate volume.

21. A few words about nebulæ. Astronomers and physicists, from the time of Sir William Hershel up

to the present, look upon nebulæ as upon the infants of the starry heavens. A nebula to them is nothing else but a non-developed, a non-condensed star—a star in the process of development. In fact, the nebular hypothesis was suggested by these nebulæ. But what a ridiculous notion? A star in its infancy! The great astronomer, Professor Simon Newcomb, the author of the admirable work *The Stars,* actually gives us in that book: "The Life-History of a Star!" Infancy, childhood, maturity, old age, and, finally, death! Are these proper attributes of stars? I appeal to the common-sense of the generality of mankind. For our part, a nebula is nothing more and nothing less than the lesser component of a binary of the sixth type (Art. 6). It is neither young nor old, but exists. It is not in the process of development, nor will it ever be developed; a nebula it is, was, and will ever be, and will forever continue to emit great quantities of visible and invisible energy; and this energy it gets from a vast massive, cold, and comparatively at rest—Primosol, as does our sun, and as all the shinning orbs of heaven do.

There is one important fact that I wish to take notice of in connection with nebulæ.

I have before me a clipping from some news paper, and I read there as follows:—

"NEBULA AND STARS.

"(From the *American Inventor.*)

"Professor Max Wolf of the observatotry of Heidelberg has made a long series of photographs of nebulæ, with short focus lenses, and has confirmed the belief of other astronomers that most extended nebulæ are situated in spaces relatively poor in stars—in deserts as it were—and that these spaces are always on one side or the other side of the nebulæ not on both."

Now, what is the meaning of this curious fact? Is it not possible, nay, is it not probable that the space on one side of the nebulæ contains the great massive Primosol, and that the nebulæ found on one side of this space revolves around this space, that is, around the Primosol? And therefore there are no stars within that space; for within that space means within the orbit of the nebulæ, or within the orbit of the binary, and throughout the starry heavens each binary has its own space for itself, free from intrusion of other binaries. It must not be forgotten that the Primosol of an extended nebula must be enormously vast in mass, and therefore must the space around it, free from other binaries, be very great. For any binary, not at an enormous distance from it, would be affected by its gravitational energy.

22. I cannot do better to properly conclude this section of the stars than by quoting the following very significant passage from *The Stars*—

"The general fact that every star has a life history—that this history will ultimately come to an end—that it must have had a beginning in time—is indicated by so great a number of concurring facts that no one who has most profoundly studied the subject can have serious doubts upon it. Yet there are some unsolved mysteries connected with the case, which might justify a waiting for further evidence, coupled with a certain degree of scepticism. Of the questions connected with the case the most serious one is raised by the geologists.

"On the theory set forth in the last chapter, that the radiant energy sent out is balanced by the continual loss of potential energy due to the contraction, the age of the sun can be at least approximately estimated. About twenty millions of years is the limit of time during which it could possibly have radiated anything like its present amount of energy. But this conclusion is directly at variance with that of geology. The age of the earth has been approximately estimated from a great variety of geological phenomena, the concurring result being that stratification and other geological processes must have been going on for hundreds—nay, thousands of millions of years. This result is in direct conflict with the only physical theory which can account for the solar heat.

"The nebulæ offer a similar difficulty. Their extreme tenuity and their seemingly almost unmaterial structure appear inadequate to account for any such mutual gravitation of their parts as would result in the generation of the flood of energy which they are constantly radiating. What we see must, therefore, suggest at least the possibility that all shining heavenly bodies have connected with them some source of energy of which science can, as yet, render no account." (Pp. 223-224.)

By "one who most profoundly studied the subject," I dare assume, Professor Newcomb cannot mean studied the subject from nature—from facts presented by nature and interpreted in a natural way—for these all point the other way. These, I say, most loudly proclaim their protest against this antropomorphic doctrine—against the doctrine of infancy, childhood, maturity, old age, decay, and death of binaries, of worlds, and of the universe—against the doctrine of evolution and dissolution of worlds. A varying star has always varied and will always vary; and whenever it is at the same point of its orbit, it is in the same stage of development or decay—in the same condition—as it has ever been; no sign of evolution or dissolution are to be found there. A nebula has always been and will forever remain a nebula. In the memory of mankind, not a single nebula has shown any disposition toward development: from an infant to become a child; from a nebula to become a star.

In the memory of mankind, the earth is what it was, and what it will ever be; in the memory of mankind, the sun is what it is, what it was, and what it will ever be. In the memory of mankind, the stars are ever the same.

I know the fable of the roses and their gardener. I know that the roses said, "in our memory our gardener remains always the same." But, let me say, were the roses provided with telescopes, micrometers, photometers, spectroscopes, and photographing instruments, and were they to use them diligently and properly—they would not fail to detect that their gardener has not, even in their memory, remained the same; that, in fact, a change either of evolution or dissolution has taken place in him!

The frequent recourse of astronomers and physicists to this fable of the roses is in itself proof that they, the astronomers and physicists—the roses—have, in spite of their being provided with the instruments mentioned above, and in spite of the incessantly laborious application of these instruments by them to the examination of the universe—their gardener—in spite of all this, they have absolutely failed to detect the slightest change either of evolution or of dissolution in it; and in their memory—in the memory of mankind (and mind you, the memory of mankind extends not to thousands but to hundreds of thousands—nay, to thousands of thousands of years: for the memory of mankind assimilates to itself the memory of the world in the shape of stratification, fossils, and planetary motions, etc.)—in the memory of mankind the universe has remained the same.

What Professor Newcomb means by "studied the subject" must refer, then, not to a study of nature with reference to the question at hand, but to a study of the principles involved in it; principles—established principles—among which the law or principle of conservation of energy stands out prominently.

Now, where a principle deduced from limited observation conflicts with the testimony of a whole universe, as Professor Newcomb, himself, is almost contrained to admit; and especially if the testimony of that universe does not necessitate a denial of that principle within the limited sphere of those observations; if it only necessitates the denial of the extension of that principle to phenomena without the pale of those observations; phenomena peculiar in their nature, distinct and different from those observed—will it not be reckless obstinacy and wilfulness on our part to persist in maintaining that extension of that principle, in utter definance of the whole universe?

I have nothing more to say, dear reader. Judge for yourself, whether the whole universe is right, or wheththe extension of the principle of conservation to gravitational phenuomonena, on which no experiments were ever made with reference to this principle—whether this xtension is right.

SECTION VII. THE UNIVERSE AS A WHOLE.

NOTHING WILL DIE.

"When will the stream be aweary of flowing
Under my eye?
When will the wind be aweary of blowing
Over the sky?
When will the clouds be aweary of fleeting?
When will the heart be aweary of beating?
And nature die?
Never, O, never! nothing will die;
The stream flows,
The wind blows,
The cloud fleets,
The heart beats,
Nothing will die.

"Nothing will die;
All things will change
Through eternity.
'Tis the world's winter;
Autumn and summer
Are gone long ago.
Earth is dry to the centre,
But Spring a new comer—
A Spring rich and strange,
Shall make the winds blow
Round and round,
Through and through,
Here and there,
Till the air
And the ground

Shall be filled with life anew.
The world was never made;
It will change, but it will not fade,
So let the wind range;
For eve'n and mor'n
Ever will be
Through enternity.
Nothing was born;
Nothing will die;
All things will change."

TENNYSON.

ALL THINGS WILL DIE.

"Clearly the blue river chimes in its flowing
Under my eye;
Warmly and broadly the south winds are blowing
Over the sky.
One after another the white clouds are fleeting;
Every heart this May morning in joyance is beating
Full merily;
Yet all things must die.
The stream will cease to flow;
The winds will cease to blow;
The clouds will cease to fleet;
The heart will cease to beat;
For all things must die.

All things must die.
Spring will come never more.
O, vanity!
Death waits at the door.
See! our friends are all forsaking
The wine and merrymaking.

We are called—we must go.
Laid low, very low,
In the dark we must lie.
The merry glees are still;
The voice of the bird
Shall no more be heard,
Nor the wind on the hill.
O, misery!
Hark! death is calling
While I speak to ye,
The jaw is falling,
The red cheek paling,
The strong limbs failing;
Ice with the warm blood mixing;
The eyeballs fixing.
Nine times goes the passing bell
Ye merry souls, farewell.

The old earth
Had a birth,
As all men know
Long ago.
And the old earth must die.
So let the warm winds range,
And the blue wave beat the shore;
For eve'n and mor'n
Ye will never see
Through eternity.
All things were born.
Ye will come never more,
For all things must die."

TENNYSON.

1. Tired and exhausted, we allowed ourselves the pleasure of luxuriating upon the grandeur and beauty

of these two truly magnificent poems of Tennyson; and, because they have a strong bearing upon some questions to be touched upon in this last and concluding section of our work, we decided to introduce them at the heading of it.

The reader will at once see that the second poem is that sung by the physicists in accordance with their present conception of things—in accordance with the extended principle of conservation; and that the first is that sung by nature under the guidance of our cardinal law of a dynamic curve.

A friend of mine properly and truly remarked that the first poem expresses the wishes, the inner cravings, the hopes—the hopeless hopes—of the poet; while the second represents the fact, the reality of things.

Our theory at once changes these conditions of things: our law given, and the hopeless hopes, and the inner cravings and yearnings of the poet's soul, become a reality—a necessity. Our law given, and the earth and the planets, the sun and the stars—visible and invisible—all together and in one accord, in one enormous chorus, eternally sing the sublimest, the most truthful, and the most beautiful, song:

"All Things Must Live"

and another:

"All Things Cannot Die."

2. What a contrast between the view presented to us by the universe when the broad and generous light of our theory is shed upon it, to that presented by it when the weird, ghastly and spasmodic glare of the arbitrarily extended principle of conversation rests,

as a curse, upon it. Why, the very heart and soul of men, of scientific and non-scientific, shudderingly recoil within themselves at the mere thought of a universe destined to destruction—inevitable destruction! A given quantity of life principle—of energy—constanttly spent, constantly dissipated, and becoming less and less—is this a principle upon which a world, a world of worlds—a universe—*is* or *was* built? Conservation! Why, it is the very demon of destruction! Imagine the governor of a city to have issued an order prohibiting all ingress of food-stuffs—life principle—into the city, and at the same time compelling the citizens to squander the food-stuffs present in the city in all possible ways—will you call that order, that law—an order, a law of conversation?

The author of the "Unseen Universe" has very ably expressed himself in a similar way respecting this principle. He says:

"So far the Genius"—the principle of conservation—"which they"—the physicists,—"had summoned up, appeared to be the very principle of order. But things wore a different complexion as time went on . . . . it was extremely startling when the Genius invoked, not content with what he had already devoured"—immortality of soul—"insisted upon the ultimate sacrifice of the visible universe—*then* the most extreme partisans of the school (of science) began at length to be alarmed. It was too much to be borne, that a Genius summoned up in the very name of order should turn out to be such an insatiate demon as this. Must the whole visible universe, indeed, arrive at such a state as to be totally unfit for the habitation of living beings? The individual they were content to sacrifice, perhaps even the race; but they would spare the universe.

"Undoubtedly . . . . they at length got themselves into a deplorable dilemma. For, indeed, the principle they had invoked was absolutely without pity, and in the most heartless manner continued to insist upon the sacrifice of the visible universe. This they were told was only a huge fire, and must ultimately burn itself out, nothing would be left but ashes—the dead and worthless body of the present system . . . . We sympathise with their perplexity; nay, we go further, and assert that to imagine the whole universe to come to an end is a monstrous supposition" (Art 83-84).

Monstrous, indeed, it is, and like a monster it frightens the heart and soul of men and they involuntarily recoil within themselves; but once within themselves, they are at peace, for an inner voice, a voice arising from the heart of hearts and from the soul of souls, a voice instinctive and full of prophecy (though not of knowledge) reassures them that such a consumation of things does not, and will never, take place. But how? and in what manner? Why, the principle of conversation *is* here. They begin to reason and drift off into "unseen spiritual worlds" as the author of the Unseen Universe has done; or fill sapce—the ether—with an inexhaustible store of enehgy, as Professor Simon Newcomb has suggested. (See *The Stars,* pp. 225).

3. It is wonderful how prolific and fruitful in inventing things, the existence of which you can neither affirm or deny, the mind of man becomes, when pressed into a corner, perhaps, by other inventions of its own. The nebular hypothesis, the meteoric theory, the contraction theory, special cometary substance, a repellent force inherent in the sun, meteoric fragments

clothed with vegetation, hot and cold regions of space, an unseen world of a spiritual nature, and space filled with an almost inexhaustible store of energy—all these are very ingenious inventions of the human mind perplexed and pressed into a corner by another invention of its own—the extension of the principle of conservation to gravitation! These are the doubtful and illegitimate children of the illegitimate principle. *Doubtful,* because to these assertions you can neither say "yes" nor "no"; *illegitimate,* because they are not the fruit of a legitimate marriage of reason to facts, but of the illegitimate marriage of reason to fancy and imagination.

4. More than this, not only were truly great men compelled to resort to invention in order to bring certain phenomena to be in harmony with this extended principle of conservation, but in many other things, where their inventive power has failed them—things, phenomena, the explanatory cause of which could not be shifted away to a remote past or to distant spaces—phenomena that occur in the present and within reach of us—they were compelled to confess ignorance and relegate such phenomena to the province of mystery.

5. Our theory—one single law: the law of a dynamic curve, coupled with a clear comprehension of the nature of gravitation and its peculiarities—a law, not assumed, but necessarily deducible from vigorous reasoning applied to simple experimental and observational data, even a priorily to an examination of the phenomena presented by the totality of the universe (See Book IV)—our theory, I say, not only renders the universe a universe, for a universe containing within itself the elements of its own destruction can

not properly be called a universe; but it renders this universe uniform throughout: no special times, no special spaces, no special substances, and no special forces—all is uniformity. What *was*—that *is*, and that *will be*, here, there, and everywhere. Our own earth is as much variable as the remote star Eta Aquillae or Omage Ceti. It, the earth, also has its winter maximum and summer minimum precisely as do those stars; and for the same reason. The comets become correlated to the sun and stars on one hand, and to the earth and planets on the other hand. All stars, all suns, all worlds, become built on one and the same necessary plan: the plan of Two; and our sun assumes its proper place among them all. What a grandeur; what a beauty; what a uniformity! The *uniformity of nature,* indeed!

The physicists boast of the principle of uniformity of nature. It is their stronghold; it is their idol; it is their searching light. But how sorely have they sinned against this principle. A world that *is* but *was* not and is not going to be; a world full of special times, special spaces, special substances, and special forces—is hardly to be called uniform. And to say of such a world "the uniformity of nature" seems to me to be mere mockery.

6. Our theory presents another attraction—an attraction touching rather the heart than the mind of men. It consists in this, that it renders the universe wholesome—free from disease and decay. What a comfortable and consoling feeling it generates to think of the world we live in as of a perfect and wholesome world. On the other hand, what a painful feeling of misery does it cause to think of our habitation—our world—our universe—as diseased, as affected with a

fatal and incurable malady. Swift, in his famous satire on mathematicians, pictures to us a city—Laputa—whose inhabitants busied themselves with mathematics. In describing their traits he says: "When they meet an acquaintance in the morning the first question is about the sun's health, how he looked at his setting and rising."

Now, we say, were the Laputans familiar with our theory they could have no cause for disquietude and fretfulness on the scope of the delicate health of this sun.

7. A most important and sublime feature of our theory is, that it not only correlates all possible modes of force or energy one to the other, but it unifies them: it proves them to be only temporary derivatives of *one* aboriginal, necessary attribute of matter—derivatives of one inexhaustible, constant, infinite, and eternal force—the force of all forces as I term it—in a word, universal gravitation! Light, heat, electricity, magnetism and chemical affinity are only so many modes of transformation of one and the same force—universal gravitation.

Little do we dream while driving in a railway carriage, for instance, that it is the Primosol, or rather the gravitational energy of the Primosol—that huge, cold, and invisible body, billions of miles away from us —that carries us along. The genial warmth and light of the sun, which light and heat constitute almost the sole source of energy here on earth, are in reality blessings sent to us by that Primosol *through the sun.* The vast, almost immeasurable store of energy in the body of the sun is, as I conceive it, nothing else but so much gravitational energy; gravitational energy, not of the sun, indeed, but of the body governing the sun

—the Primosol. And since all forms of energy are, in the last analysis, derived from the sun's rays, therefore, we may truly say that all forms and manifestations of energy here on earth are, in the last analysis, nothing else but so much Primosolic gravitation. It is the gravitational energy of the Primosol that makes our plants grow; it *is* it that renders life and growth of all living beings, including humanity, possible; it is *it* that causes rain and snow, thunder and lightning, winds and storms and ocean currents on earth; it is *it* that supplies the energy necessarily expended in thought and feeling. In a word, *it* is the source and efficient cause of all change and activity here on earth.

8. This unification of all forms of energy in gravitation naturally leads the mind to, and proves to it, a grander, higher, and most sublime *unity*—a unity which unifies all unities; a unity in which matter, inertia, and gravitation, are unified; a unity of which matter, inertia, and gravitation, are but attributes, and the whole universe but a symbol; a unity of which the human mind is incapable of forming any conception whatever, for it is the unity antecedent to all unities; a unity which embraces not only matter and its attributes but also mind and its attributes—in a word, a unity of the first and supremest cause—of God!

9. I said that, outside of gravitation, all other forms of energy are but temporary and derivatives of it, meaning thereby that while gravitation is constant and abiding, light, heat, electricity, etc., originating from gravitation, have but a temporary existence. Far from predicating conservation of these limited and exhaustible forces, we predicate of them just the contrary: temporarity; for such is the bare fact

staring us, as it does, squarely in the face. In the last analysis, all these forces are converted into heat, and heat is rapidly dissipated into space: that is to say, it is gone, destroyed, and annihilated, as far, at least, as our solar system is concerned.

10. The physicists are bound by the principle of conservation to consider the dissipation of energy not as absolute loss, destruction, or annihilation of energy; but as a transportation and subdivision of energy. They believe, namely, that the heat (molecular motion) radiated away from a heated body (say the sun) into space, continues its existence as molecular motion of some form and of some intensity in that space and never ceases to exist.

Now, in accordance with our theory, another view of the matter may, perhaps with more reason, be entertained. Imagine a body of unit mass, moving with a velocity V, to come in collision with another body of the same mass, at rest. The two bodies will then move together with a velocity of $\frac{V}{2}$. If these two bodies be supposed to have again come into collision with a third body of unit mass, then, will the conjoint velocity of the three bodies be $\frac{V}{3}$. In general, if the collisions as here described be supposed to have been repeated M tims, then will the conjoint velocity of all the M+1 bodies be equal to $\frac{V}{M+1}$ and if M be very large, this conjoint velocity will be very small. But since there is a least possible velocity, a microf, it becomes evident that this process of the subdivision of the motion (V) between a larger and larger number of bodies must finally lead to an absolute destruction and annihilation of the molar motion, as far as molar motion is concerned. The molar motion thus destroyed may or may not continue its existence as molecular motion; but as far as molar motion is concerned, it is absolutely and positively destroyed.

Similarly with molecular motion, in its turn. With the increase of the number of molecules among which a given amount of molecular motion is subdivided, the molecular motion itself gets weaker or diminished in some respect, whether it be in velocity, amplitude, or some other quality. And if there be a minimum limit to this velocity or amplitude, etc., then will this process of molecular collision and subdivision of molecular motion finally lead to an absolute annihilation and destruction of that motion—to a cessation of existence of that motion.

11. If a pebble be tossed upon still water, it crowds aside the particles beneath it and raises them above the level, thus forming a wave around it in the shape of a ring. This elevated ring of water descending, raises another ring of water outside of itself, that is to say, it communicates its molar motion to a ring of water larger than itself. (For the sake of argument assume that the whole of the molar motion of the first ring is communicated to the second.) The mass of matter in the second ring being larger, the molar motion of each particle of the second ring must be smaller than that of the first; and therefore is the height of the second ring *less* than that of the first. In a similar way is the molar upward motion of each particle of the third ring less than that of the second; that of the fourth less than that of the third; and so on, until a ring is finally reached whose mass is so large that the original amout of motion communicated to the

water by the pebble is just sufficient to give each particle of this last ring the minimum limit of velocity: a microf. Each particle of this last ring, coming into collision witth many particles of water at rest outside of the ring, is finally itself arrested in its motion, without having communicated it to the outside, for a velocity of less than a microf is an impossibility.

Thus we see that the original motion, after having been communicated to larger and larger masses, is finally, when it reaches the ring with the least possible velocity, destroyed and annihilated. The tossing of pebbles one after another in the centre of this ring may be ever so rapid, yet not the slightest amount of molar disturbance can be manifested outside of this limit ring: all the motion of all the pebbles will be destroyed within this ring.

This description of the production of a fluid wave is not intended to describe the process as it really is; it is only intended as a rough illustration of what, in our opinion, may be the case with the ethereal disturbance produced by an incandescent luminous object.

As we conceive it, the luminous object first produces a molecular disturbance in the ethereal molecules in immediate contact with it, which, if the luminous object be of spherical shape, constitute a spherical shell around it. This shell promptly hands over the disturbance to the shell of molecules immediately outside of it. The second hands it over to the third shell. The third to the fourth; the fourth to the fifth, and son on. Now, as the surface of a sphere is directly proportional to the square of the radius, it is easy to see that the number of molecules of any spherical shell when compared with that of another will be as the square of the radius of the first shell to that of the second (the luminous object constituting the centre of all these concentric spherical shells); and therefore, will the intensity of the molecular disturbance of the spherical shellss be inversely proportional to the square of their distances from the luminous object (which, in fact, is the case, as is very well known); for, a given amount of disturbance must be inversely proportional to the number of molecules among which it is subdivided.

We thus see that the intensity of the molecular disturbances of the ether produced, say by a single pulsation (vibration) of a luminous point, rapidly decreases as it recedes from that point outward. And if, as is the case with molar motion, there be a minimum limit to the intensity of a molecular disturbance; if there be, in other words, what we may call a *molecular microf*—then, a spherical shell will finally be reached in which the number of molecules will be so great, that a single pulsation of the luminous point will be just sufficient to give each of the molecules of this shell a molecular motion (disturbance) equal to one molecular microf. And since the molecules of this limit shell must, while in motion, inevitably come into collision with a greater number of molecules of the shell outside of them; and since they cannot hand over their molecular motion to them, because a motion less than a *molecular microf* is, by supposition, an impossibility—therefore, will the molecular motion of the limit shell be finally arrested, that is to say, destroyed and annihilated absolutely. And as the luminous object sends out one pulsation after another, these will all reach the limit shell one after another to be here finally destroyed; and not the slightest molecular disturbance will manifest itself outside of this limit shell.

12. If the above be true, then it comes out that

the activity (thermal activity) of a sun extends only across a limited sphere. The magnitude of the sphere—its diameter—is and may be of enormous vastness. Tens of years—nay, hundreds of years may be required for light to travel from that sun to the boundary of the sphere—to the limit shell; but limited it is. And if an observer be placed outside of this sphere, he cannot possibly become aware of the existence of that sun or star, since no activity from that sun can possibly reach him. No matter by how many times he may increase the magnifying power of his telescope, that star, outside of the sphere of activity of which he is placed, cannot become visible to him.

13. As it is by virtue of our theory that we arrived at the above conclusion; for, since by our theory finite energies are constantly being generated de novo, it behooves us, for obvious reasons, to conceive of them as also constantly being lost, destroyed, and annihilated at the other end of their existence—therefore, it is very gratifying to find that from entirely different considerations, from factual considerations, in fact, a great man—Sturve—has arrived at nearly the same conclusion more than half a century ago.

"About half a century ago Sturve propounded the contrary hypothesis that the light of the more distant stars suffers an extinction in its passage to us." (*The Stars,* pp. 231.)

Sturve, true to the principle of uniformity of nature, assumed that the stars were on the average uniformly distributed through infinite space. And on the strength of this warranted assumption he beautifully proved in a mathematical way, that were the light of *all* the stars actually reaching us, no matter in how faint a degree; that were not the light of those very distant from us suffering absolute extinction before it reaches us; in other words, that were there no limit to the sphere of activity of a star; were there no such thing as what we called a *molecular microf*—then, would the whole heavens "be filled with a blaze of light as bright as the sun"; the whole heavens would, then, have an aspect analogous to that of the milky way, only the light would be much more intense.

Modern physicists, on the other hand, true to their idolized extended principle of conservation, could not, of course, agree with Sturve. But why are not the heavens "filled with a blaze of light as bright as the sun?" Rather than limit the extent of the sphere of activity of a star and thus infringe upon the principle of conservation, they, the physicists, actually dared to limit the extent of the universe; and they say the heavens are not filled with light because there are no stars in infinite space to supply that light; because this universe is limited in space!

O, what an awful demon of *destruction* this insatiate extended principle of *conservatism* is! Not satisfied with dooming the whole universe to final destruction, as far as change and activity, life and thought are concerned; not satisfied with robbing it of time—it also robs it of the freedom of space, by confining it to a comparatively small space within the infinite and boundless space! Not satisfied with attacking its spirit—its energy—it also attacks its body—its matter! (See *The Stars,* pp. 226-232.)

14. We cannot leave this subject without hinting at another very important possibility—nay, probability. It is this: If the sphere ofactivity of a radiating body is limited; if there is a minimum limit of inten-

city to finite energies; if this minimum intensity of finite energies cannot further produce any effect on other bodies—is it not possible—nay, is it not probable that there is also a minimum limit of intensity to gravitation; that a gravitational intensity less than minimum cannot produce any effect on bodies; that, in other words, the sphere of the gravitational activity of a body is also limited, and beyond that sphere, beyond a limited distance from the gravitating body—it, gravitation, ceases to be effective? We have good heason to answer this question in the affirmative; but as this question has very little direct bearing on our theory, we prefer to leave it entirely unanswered, being satisfied with the mere allusion to it.

15. A few words about the luminiferous ether will not be out of place here. Since molecular motion of material bodies produces molecular effects upon the ether, or rather upon the ethereal molecules, therefore, must the particles of ether be concived as possessing inertia—molecular inertia: that is to say, as offering resistance to molecular motion, for, by proposition I of Section III, Book II, on no resistance force cannot possibly act.

On the other hand, the ethereal molecules can not be conceived as possessing *molar* inertia; for, if they did, the ether would offer resistance to the inertial motion of the suns around their Primosols, and this would inevitably lead to a destruction of the universe, as is self evident. But the universe does exist, and, moreover, we consider it an axiom that an existing universe cannot contain within itself the seeds of its own destruction.

It comes out, therefore, that the luminiferous ether is a substance entirely different from what we ordinarily call matter. While matter possesses molar *and* molecular inertia, the ether possesses molecular inertia only, but no molar inertia: it is entirely transparent, so to say, to molar motion.

Further, as gravitation has reference only to molar motion and not to molecular motion, as was repeatedly said in previous pages—therefore, can gravitation produce effects on ordinary matter only, which is endowed with molar inertia; but it cannot possibly produce any effect on ethereal molecules which do not possess molar inertia. And it is thus seen that our forefathers were nearly correct when they termed the molecular energies "imponderables," for these molecular energies are sent to us through the ether, which is, indeed, *imponderable.*

The universe, then, as far as its substance is concerned, consists of two kinds of substances; one, matter, possessing both molar and molecular inertia; the other, ether, possessing molecular inertia alone.

We might also call ordinary matter *gravitational substance,* while ether we might term *non-gravitational substance.* And the plan of the universe is the existence of gravitational substance in a non-gravitational medium. What a wise plan!

16. One thing more and we are done. Throughout this work we have fiercely attacked the principle of conservation; on the other hand, the main arguments upon which our theory rests, have this very principle as a basis. And it behooves us, therefore, to define our position clearly. We say again, as we said in another place, that the principle of conversation, as far as it is applicable to *existing* limited energies: light, heat, electricity, etc., in their action on bodies or in their interaction among themselves, is a positive fact, and

there can be no controversy about it at all. But, we say, that this is true only as long as gravitation is not a factor in that action or interaction. (See Art. 27, Sect. I, Book I). We further wish to remark that even within the sphere of the limited energies conservation is only true within the sphere of activity of a star or sun at any point between the centre (the sun) and the circumference of that sphere; at either end of this radius it is not true, for at the centre, energy is generated de novo, and at the circumference it is annihilated. Of course, even at any point between these two ends finite energy may be annihilated by gravitation, as when a body is thrown vertically upward. But in this case there is always an "advantage of position" —the potential energy of the physicists—or a possibility for the same amount of energy to be regenerated de novo by the same gravitational body which annihilated it. So that, in truth, throughout its journey from the sun to the circumference of the sphere of activity of that sun, finite energy may be said to be conserved, if this "possible energy" be taken into account.

More, it is also true that the average quantity of finite energy in the universe is constant; for, in the average, as much finite energy as is generated in the sun, just as much is annihilated at the circumference; and thus the quantity of energy present within the sphere of activity of any sun is, on the average, constant. And in this sense, in this sense only, it may be said, with a certain looseness in language, to be conserved.

Thus understood there is nothing truer than the principle of conservation.

The physicists, attempting to converse energy, have destroyed the universe—have limited its existence both in time and in space, and have necessitated the paradoxical anticipation of a time when the universe will be gone and its energy present.

Our theory, on the other hand, has succeeded in conserving both the universe and its energy (in a sense).

We cannot conclude our work in a fitter manner than by pointing out to the reader that what we really endeavored to prove throughout this work, from the beginning up to this end, is actually nothing more nor less than that

"In His goodness He every day—constantly—renews the works of Creation."

FINIS.

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NOTE ON RADIUM.

The mystery of Uranium, Poloium, Actinium and principally of Raduim, the mysterious radio-activity of which so agitates the whole scientific world at present—seems to me to be perfectly and naturally explicable by our theory:

The atoms, or molecules, or radicles, of the radio-active substances may be conceived as consisting of two parts revolving in orbits, circular or elliptical, around their common centre of (cohesive) gravitation. And therefore, like the two components of a binary in the starry heavens, they (both parts of the atom) radiate energy into surrounding space, because of their motion in dynamical curves.

I said "(cohesive) gravitation" because I look upon the cohesive force (including chemical affinity?) as a kind of gravitation—molecular gravitation, we might term it, in contradistinction from molar gravitation.

This explanation of the radio-activity of radium may appear bold and daring to those who have not assimilated the truth of our theory into their mental constitution; to us it appears simple and quite natural. Radium is positively radiating energy. Wherefrom is that energy taken? The theory that the radium radiations are caused by minute particles of matter flying off from it, with a light-velocity, in all directions, appears to us not to touch the question at all. It only assumes the *method* of radiation, but not the cause of it: it does not give us the source of the energy; for, undoubtedly, energy must be spent for setting these assumed particles into motion.

The following, from an article by R. De C. Ward, in "Science" (Sept. 4, 1903), is highly interesting in this connection.

" . . . . Other curious points in the motion of electrons are the following":

"Uniform motion in a straight line once established is steady and permanent. Uniform motion of an electron in a circular orbit, on the other hand, is accompanied by continuous radiation of energy, so that such motion must eventually cease if it is not maintained by some external agent. Thus two electrons rotating about each other after the manner oof planet and satelite slowly radiate energy, and their motion is affected very much as if they were in aa resisting medium."

We do not care to know what really constitutes the "electron": perhaps it is really one part of the binary atom or molecule; perhaps it is the ether itself; but whatever it is, when it moves in a dynamic curve it "slowly" radiates energy. That "such motion must eventually sease" I assume to be only an inference on the strength of "conversation," and not a matter of fact. If so it may be dissented from.

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